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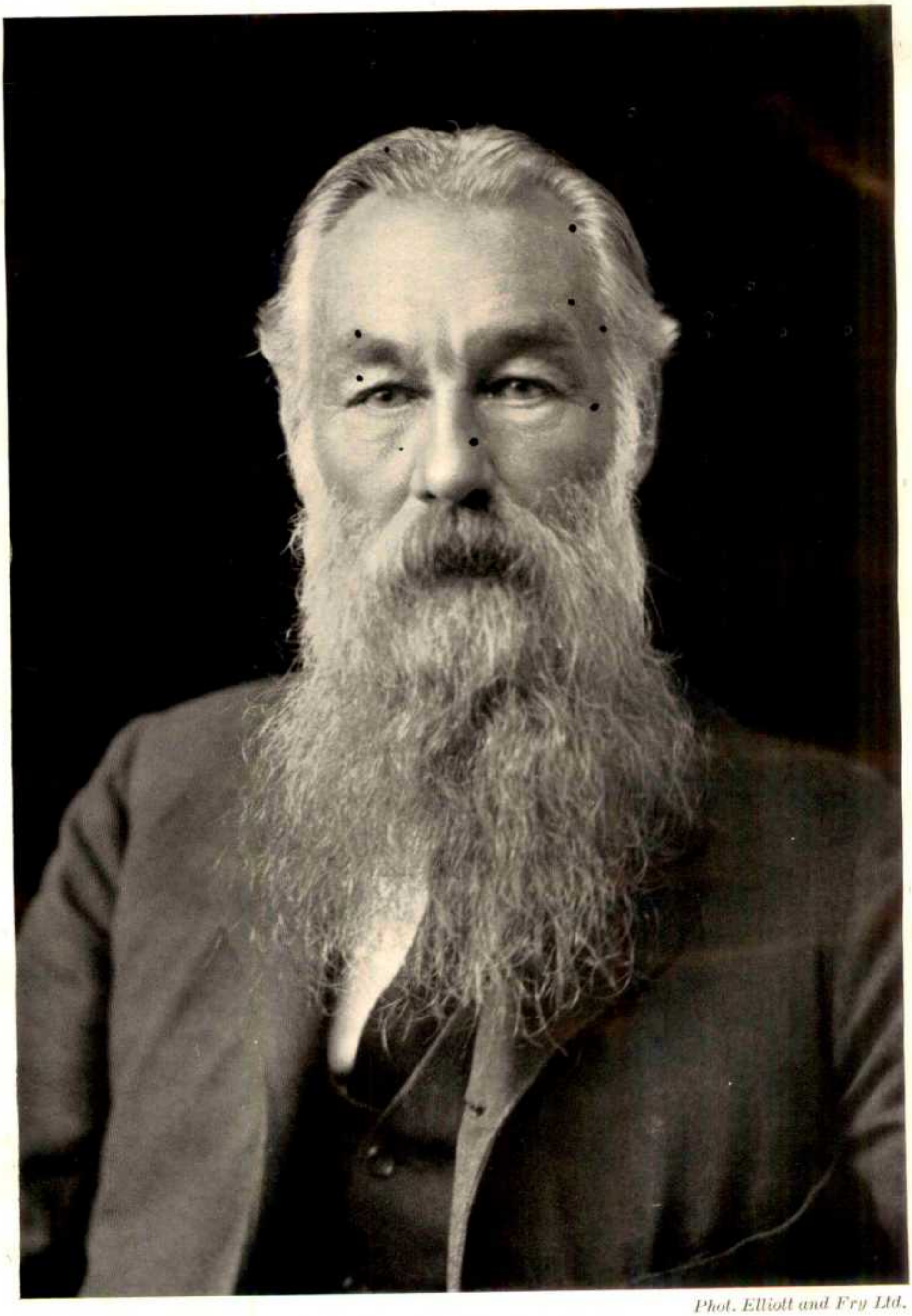

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PROF. C. LLOYD MORGAN

VOLUME XXVII JULY, 1936 PART 1

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OBITUARY NOTICE

PROFESSOR C. LLOYD MORGAN

1852–1936

By the death of Prof. Lloyd Morgan, at the age of 84, English psychology has lost one of its outstanding figures, and one of the pioneers in the study of animal behaviour.

Born in 1852, the son of a London solicitor, Conwy Lloyd Morgan received a classical education at the Royal Grammar School, Guildford, and then—intending to be a mining engineer—he entered the School of Mines in the Royal College of Science. Here he had a brilliant career, winning medals, scholarships, and an Associateship in Mining and Metallurgy, and reading widely especially in philosophy and biology. At the end of his college course he had leisure to tour in America, where, under the influence of Darwin's writings, he became more and more attracted to biological science. He returned to the Royal College to work under Huxley, and it was on Huxley's advice that he began to study instinctive behaviour in animals, a subject upon which he was later to become the foremost authority. In 1878 he took a lecturership at Rondebosch College, South Africa, where he taught with success a surprising combination of subjects which included English, constitutional history, and physics. Five years later he returned to England to succeed W. J. Sollas in the chair of Geology and Zoology in the University College of Bristol.

At Bristol, where he was to remain for forty-three years, he threw himself wholeheartedly into the task of developing the then small University College. In 1887 he succeeded Sir William Ramsay as Principal, and held that office until the College became a University in 1910. During this time Lloyd Morgan devoted an immense amount of energy to all sides of the work of the College, and some of his colleagues have confessed that, had it not been for his charm of manner and his ready sympathy, it would have been a little alarming to work under a Principal whose detailed knowledge of every subject was so great. In the town he was a familiar and respected figure, who could be seen every day cycling to work, to golf, or to Church, with no overcoat in any weather, and with his long beard resting on the handlebars, or blowing wildly over his shoulder.

It is remarkable that nearly all Lloyd Morgan's work in psychology was done during this period, when he was the energetic principal of a growing University College. Perhaps it is a pity that administrative work did not allow more time for his own studies; but, even so, his output was astonishing. In *Animal Life and Intelligence* (1890), *Animal Sketches* (1891), *An Introduction to Comparative Psychology* (1895), *Habit and Instinct* (1896), *Animal Behaviour* (1900) and numerous papers in scientific journals, he took the results of his own and other workers' observations on animal behaviour and welded them into a coherent view of the relation between instinctive behaviour and intelligence, a view which he developed in greater detail in *Instinct and Experience* (1912). In addition he published *Animal Biology* (1887), *Psychology for Teachers* (1895), and *The Interpretation of Nature* (1905). In 1899 Lloyd Morgan was elected a Fellow of the Royal Society: he was the first Fellow who had ever been elected for psychological research. His work on animals was always notable for the carefulness of his observations, and for his vivid descriptions of behaviour. Moreover, he was one of the first psychologists to see the need for experimental, as well as observational, treatment of the problems of learning; and it has often been said that modern animal psychology grew out of Lloyd Morgan's work in England and the early work of Thorndike in America. But perhaps the feature of his writings which was historically the most important was his extreme caution in the interpretation of results, and the sharp distinction between the observed facts and the inferences to be drawn from them. His famous "Canon", enunciated in 1895, namely that "in no case may we interpret an action as the outcome of the exercise of a higher psychical faculty if it can be interpreted as the outcome of one which stands lower in the psychological scale", may now appear to be an unnecessary admonition to follow the only possible path; but a glance at some of the earlier writings will show the salutary nature of the revolution in thought which Lloyd Morgan did so much to bring about. No one has written more sensibly about the animal mind, for Lloyd Morgan had no bias either towards humanizing or towards automatizing. The behaviouristic trend of animal psychology since then has diverted attention from those problems of the nature of an animal's conscious processes in which Lloyd Morgan was so much interested; but the problems are only shelved temporarily, to be attacked again when the results of human introspection become more coherent.

In 1910 the University College of Bristol became a University, and Lloyd Morgan was the first Vice-Chancellor. His work of development done, he retired after three months, and most of the rest of his life was devoted to philosophy. He developed the theory of "Emergent Evolution"—the theory, that is, that at certain levels of complexity in the evolutionary process new properties suddenly emerge; and it was characteristic of him that he should first put forward this theory in an address to a Workers' Educational Association Summer School. The theory of emergent evolution has occupied an important place in modern thought, and in 1926 Lloyd Morgan became President of the Aristotelian Society.

Few men can claim such a record of varied achievements. Lloyd Morgan was an accepted authority on many branches of knowledge, and an inspiring teacher. He took a leading part in the development of a University. He did much to influence the progress of psychology, and to place animal psychology on firmer foundations. He made important contributions to philosophy. But with all his activities, he always had time to take a keen personal interest in his younger colleagues and students; and he will be remembered by them for his kindness, his sense of humour, and his sound judgment.

G. C. G.

THE LEARNING OF LOGICALLY CONNECTED MATERIAL

By R. C. OLDFIELD
(*Psychological Laboratory, Cambridge*)

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    (1) *Experimental* (pp. 6–7).
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I. INTRODUCTION

ATTEMPTS to investigate the process of thinking fall in general into two types, namely those in which, by introspective or behaviouristic methods, we endeavour to discover the characteristics of processes that occur at the time when an individual is engaged in solving a problem, and those in which we try to enquire into the prerequisite conditions that must be fulfilled in order that the process of thinking shall occur, and shall reach a conclusion. Under this latter head there are questions of whether or not the individual is in possession of the relevant data for solving the problem, and whether or not that material is, being possessed, available, or suitably organized, to play its part. And in this latter connection there are further problems of the means by which the individual comes into possession of the material, and of how differences in this respect affect differences of availability and so forth. The experiment described in this paper is an attempt to throw some light upon one of these problems, namely, that of the differences between the learning of material when that material consists of a number of items that are logically independent of one another, and when it consists of a number of items that are logically related to one another.

II. MATERIAL

The material chosen was that provided by family relationships. The logical possibilities implied in such material may be assumed to be equally familiar to all the subjects (although for one reason or another some may be more apt than others in learning such material). The material for each experiment consisted of eight items of similar form. Each item consisted of a Christian name, to which was attached a particular, stating the relation of the possessor of the name to some other person, also specified by Christian names. Thus a single item was of the form

Mary...George's mother

In the first experiment, each of these items was independent of the others, that is, no name occurred more than once. In the second experiment, some of the items were logically connected to others by the introduction of the same name more than once. Thus, in the second experiment two of the items were

Susan ... Simon's wife.
Simon ... Mark's brother-in-law

In the third experiment, the material was so constituted that all the persons mentioned belonged to the same family. In the fourth, the items were arranged so that a number of inconsistencies and irregularities of family relationship were introduced. For instance,

Ruth ... Robert's wife
Arthur ... Ruth's husband

The material for the experiments was as follows.

Experiment I

Henry	...	Stephen's grandfather
Mary	...	George's mother
Richard	...	Maud's uncle
Peter	...	Joan's brother-in-law
Anne	...	Hubert's wife
John	...	James's brother
Harriet	...	Betty's aunt
Charles	...	David's cousin

Experiment II

Geoffrey	...	Harry's brother
Susan	...	Simon's wife
Mark	...	Celia's brother
Daniel	...	Susan's father
Celia	...	June's cousin
Jeremy	...	Mark's father
Thomas	...	Gerald's uncle
Simon	...	Mark's brother-in-law

Experiment III

Eileen	... Alfred's sister
Christopher	... Herbert's brother
Paul	... Joyce's brother
Lucy	... Hilda's sister
Alfred	... Paul's father
Joyce	... Christopher's mother
Hilda	... Paul's granddaughter
Herbert	... Lucy's father

Experiment IV

Victor	... Ruth's brother
Frank	... Robert's father
Elizabeth	... Arthur's wife
Robert	... Victor's brother
Frederick	... Arthur's father
Ruth	... Robert's wife
Dorothy	... Frank's daughter
Arthur	... Ruth's husband.

III. PROCEDURE

(1) *Experimental.* The subjects were told that they were to learn the particulars relating to each of the Christian names, so that when given a name they would be able to respond with the particular that went with it. In Exps. II, III and IV, they were told that responses consisting of particulars which, although true, were not the particulars originally given, would be counted as incorrect. Thus, in Exp. II, it could be inferred that Celia is Jeremy's daughter and Mark's sister, while the particular originally attached to the name Celia was that of being June's cousin. The subjects were then told that the list of names, each with its particular, would be read through once. The names would then be given in random order, and they were instructed to reply with what they believed to be the correct particular. If really unable to do this, they were to say 'No'. Guesses were allowed, and they were permitted to give such replies as 'Somebody's father', or 'Gerald's something'. In the event of a correct response, they would be told that it was correct, while if it were incorrect, they would be told the correct one. In the case of Exp. IV, they were told in addition that they would notice certain inconsistencies in the relationships of the persons concerned. The experiment was continued until the subject had given the correct response to each name three times in succession, or until fifteen rounds had been gone through. At the end of the experiment some simple questions were

asked regarding the method used by the subject in memorising the material. No extended introspections were asked for.

(2) *Scoring.* Each particular consisted in a name and a relationship. One mark was given for the correct name and one for the correct relationship. It is, of course, evident that the random chance of guessing the correct relationship was greater than that of guessing the correct name. But it was not thought worth while to adopt a more elaborate method of scoring.[1]

IV. RESULTS

The mean score for the group of subjects for each trial was plotted against number of trial. The four learning curves so obtained are shown in Fig. 1. In calculating the mean scores, it was assumed that subjects who had maintained the maximum score of 16 on three successive occasions, would have continued to do so, although in fact the experiment was discontinued at this point with any subject who reached this degree of proficiency.

In general the method adopted by the subjects in Exps. I, II and III seemed to be that of associating the names occurring with people known to them who bore these names. This method was followed even where, as in the majority of cases, the relationships would not fit. Some of the subjects, particularly in the second and third experiments, admitted that these inconsistencies between the relationships to be learnt, and those holding between the concrete counterparts assigned to the names, hampered them. In some cases, in Exps. II and III, subjects stated that at first, perceiving that there were family relationships among the names, they attempted to learn the scheme of the family as a whole, but only in few cases was a subject successful in this. In the others, the subject was compelled to fall back on the method mentioned above. In Exp. IV, this method of matching the names to known persons was only used in one or two instances, and then in a very incomplete fashion. In Exps. III and IV, responses which consisted in correct inferences from other items were given with fair frequency. Thus in Exp. III there were in all 261 responses given which were incorrect, did not consist in the answer 'No', and were not of the form 'Somebody's Uncle', or 'Gerald's something'. Of this number 76, or about 29 per cent., were correct inferences. In the great majority of these cases, the inference merely consisted in the inversion of another item. For example, in Exp. III, the response to 'Hilda' was frequently 'Lucy's sister', the correct items being

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[1] Learning curves drawn by counting only correct responses and neglecting half-correct responses were very similar to those shown in Fig. 1.

Hilda ... Paul's granddaughter

and Lucy ... Hilda's sister

In one or two cases only was a second order inference, requiring two steps, and the use of two other items, found.

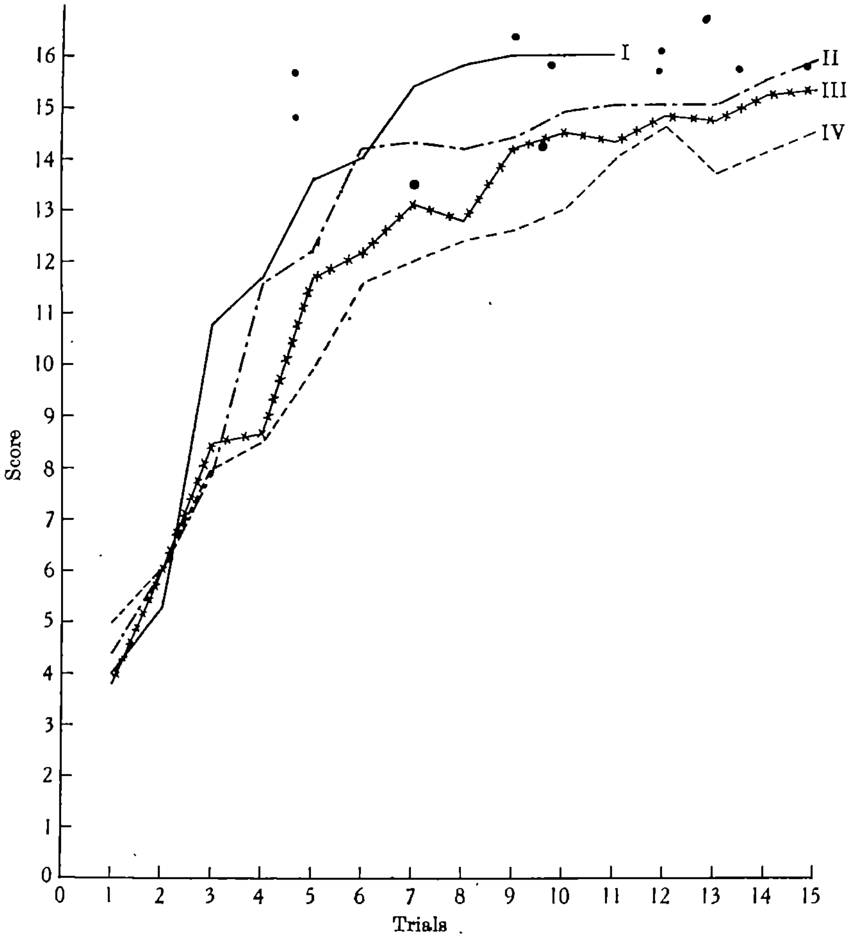

Fig. 1. I, all items independent of one another (mean of 11 subjects); II, some items logically connected (mean of 12 subjects); III, all items logically connected (mean of 12 subjects); IV, some items logically inconsistent with one another (mean of 8 subjects).

V. DISCUSSION

The general facts that may be elicited by inspection of the curves of Fig. 1 are that the learning of this kind of material is slower when there are logical connections between the various items than when the items are independent logically of one another; that the introduction of logical inconsistencies increases the learning time still more than does the presence of consistent logical relationships;[1] and that the general form of the learning curve does not appear to be altered by the introduction of logical relations; no evidence is provided by this experiment at least for the intrusion of any factor of insight into the process.

It may appear at first sight surprising that material that has a logical structure is more difficult to learn than is material consisting of a number of independent items. We are in general prone to believe that orderly, logical material is easier to assimilate than is material that is disconnected. But it must be pointed out that in this experiment, and this remark is perhaps capable of wider application, the task set the subjects is that of *learning to give the arbitrarily correct response* (*chosen by the experimenter from a number of responses which logically are equally correct*), *and not, primarily, that of learning the material as a whole,* that is, the members of a family in their family relationships. All that can be concluded from these experiments is that when material which has in fact a logical structure is presented in the form of a number of separate items, and when the task before the subject is to learn that material in the form of a number of separate items, the subject does learn something beyond this mere collection of items, namely, something of the material as a whole. This much may be inferred from the longer learning time involved, and from the appearance of inference responses in place of the arbitrarily correct responses.

We may, perhaps, tentatively conclude, that in general when we learn a number of separate facts which bear logical relations to one another (and this is, obviously, an exceedingly common occurrence in everyday life), there is a tendency for these particular facts to become merged in a general scheme representing the total state of affairs involved; so that, while we may later be incapable of reproducing the particular facts learnt, we may be said to have acquired a general reaction tendency in regard to that state of affairs. The experiments described at least indicate that logically related items are not merely learnt as separate items (in the sense that any reaction at a later date to the general state of affairs represented in the material consists in the revival of the items learnt, together with conscious inferences based upon them). On the contrary, the evidence shows that the learning of the items as items is essentially and unavoidably accompanied by some other process, which, we suggest, is a learning of the material as a whole.

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[1] Whether the difficulties raised by these inconsistencies were really due to their logical nature, or to the fact that they indicated, in the family concerned, irregularities of custom and morality, was not determined!

VI. SUMMARY

An experiment is described in which it is shown that when material consisting of a number of separate items is learnt, the process of learning is lengthened by the introduction of logical relations between the items. It is lengthened still further by the introduction of logical inconsistencies between the items. No factors of insight appear to play a part.

In conclusion I wish to thank Prof. F. C. Bartlett for his help, and for permission to work in his laboratory.

(*Manuscript received December* 17, 1935)

THE INFLUENCE OF AGE AND SOCIAL GROUP ON CHILDREN'S REMEMBERING

By MARY L. NORTHWAY
(*University of Toronto*)

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I. INTRODUCTION

THIS is an investigation of the remembering of children of different age groups and in schools with varied social backgrounds in one geographical environment, namely Toronto.[1] Using the chain reproduction method devised by Bartlett (1) and stories of different types, some general findings about children's remembering were obtained. More especially attention was directed to the difference in recall between the *age* groups and among the *social* groups. Bartlett has suggested a theory of social remembering from his experiments with widely divergent social groups. Here we endeavour to discover whether the theory is substantiated and developed when the social groups differ in their background and customs, but possess if not identical, at least closely overlapping, geographical environments.

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[1] The opportunity to secure these data has been provided by Miss M. Lowe of the Bishop Strachan School, Mr H. W. McIntosh of Orde St. Public School, Mr D. A. Glassy of Harbord Collegiate, all of Toronto, and by Mr Taylor Statten, Pickering College, Newmarket, Canada. To them, to Dr Line of the University of Toronto, who made arrangements for conducting the experiment, and to Prof. Bartlett for suggesting means of systematizing the data, the author expresses sincere thanks and gratitude.

II. PROCEDURE

(1) *Subjects*

The subjects were children from three Toronto schools, with as widely different social backgrounds as possible in one community. In two schools there were groups of 10- and of 15-year-old subjects. In the third school only one age group, that of 14-year-olds, was available. All these groups were not available for all the stories.

The *first* school is a Church of England, private, girls' boarding and day school. It has an enrolment of about 400, of whom about 300 come from the wealthy and most established parts of the city; the other 100 are boarders, from similar backgrounds. The school is one of the oldest in Canada, and has therefore certain fixed conventions and traditions. It embodies an essentially stabilized social background.

The *second* school consists of two parts: (*a*) a public school[1] of about 1000 pupils in the slum area of the city, and (*b*) the continuing high school.

(*a*) The population of this school is composed largely of children of foreign extraction, ranging from the first to the fourth generation of immigration. The backgrounds of the children are very diverse, though they belong chiefly to the lower working, or unemployed, classes. It is essentially the centre of a very heterogeneous and fluctuating population.

(*b*) The high school has a population of a slightly more stabilized type; though it includes the same district, it serves a wider area than the public school. The pupils were available for the first story only.

The *third* school is a boys' boarding school, situated just outside the city. It has an enrolment of about a hundred, is run on 'progressive methods', and has its own psychological department.

Each group consisted of children from one school classroom. These groups varied in size from thirty-two to forty, except in the country school, where only twenty subjects were available.

(2) *Material*

Three stories, each approximately 250 words in length, but varying in type, style, and difficulty were used.

(*a*) *Mary-Ann and Joseph*. This was a loosely connected, imaginative story of which the words, phrases, and incidents were all clear and comprehensible to all the subjects.

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[1] A 'public' school is the free, state-endowed school.

Mary-Ann and Joseph

One day Mr Winkleyes and Mrs Doonought had all the little boys and girls who knew them come to a tea party. After the children had tea in blue cups with deckled edges and little pink cakes with silver balls on them, Mr Winkleyes said, "Who would like to see my beetles?" and everyone said at once, "I would".

So Mr Winkleyes said, "Well, you all shall, but Mary-Ann shall first because she is the very smallest girl here."

So Mr Winkleyes took Mary-Ann to a black, shiny table, and on the top of it was a bright gold microscope. And Mr Winkleyes said, "Look, there is my favourite beetle. His name is Joseph."

Mary-Ann looked down the black tube and there was Joseph; he looked as big as a pony. When Joseph saw Mary-Ann he winked his right eye and said, "Hello, Mary-Ann, would you like to go for a ride?" and Mary-Ann said, "Yes, very much."

"Slide down the tube then and get on my back", said Joseph. So Mary-Ann did and off they started.

Then Joseph said, "Would you like to ride to that pink cloud above the farmyard?" and Mary-Ann thought that would be lovely because she had never been there.

On the way, to make conversation, Joseph said, "I don't like spinach much, do you?" and Mary-Ann said, "No, but ice-cream is nice." Then they sat on the pink cloud for some time and talked and watched the people below them. After a while Mary-Ann said, "I liked the ride very much, but we had better go home now, because it is nearly supper time."

So they came down very suddenly, as you do on the roller-boller, and Mary-Ann found herself looking down the tube at Joseph, and he winked his left eye and said, "Don't tell, my dear."

Then Mary-Ann said goodbye to Mr Winkleyes and Mrs Doonought, and Mr Winkleyes said, "You must come again another day, Mary-Ann, and perhaps Joseph will take you to the clouds again."

(*b*) *The War of the Ghosts.*[1] This story, which has already been used with adults, is more difficult in words and context than the former story.

(*c*) *The Finals.* This was a 'weighted' story. It combines items highly meaningful in the social and academic life of one school but quite neutral in meaning for other groups, with items highly saturated with meaning for other groups in different schools. It also combines two separate series of events; these have a common start and finish, but run divergently through the story. The 'weighted' words are italicized in the copy below.

The Finals

Final games are always exciting, but when two members of one family are playing on teams the enthusiasm is especially high. It happened one day that Patricia Woodworth was playing in the final *basketball* game at her school, while her brother, Lloyd, was *captain* of the *rugby* team at his school. They left in the morning in great spirits and Mr Woodworth wished them the best of luck.

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[1] For the complete story see Bartlett (1), p. 65.

The morning seemed very long at both schools. After *chapel* Patricia, who was in *V middle science*, had *Holy Scripture* and *sewing* before *recreation*. During *Algebra* she was so restless with excitement that the mistress gave her an *order mark*. Kay, who was her best friend, had a *detention* because she had not learned her *catechism*. Meanwhile Lloyd at his school was equally excited. After *assembly* he had *civics* and *geometry* and later *P.T.* and *manual training*. All this time he and the other members of the gang kept thinking of the *signals* and hoping they would not forget them. He was in *second form* and their room was next door to the *sight saving* class.

At last noon came. Patricia went to *roll call* and she was very pleased and surprised to be given her *school colours* by the *Head Mistress*. Lloyd had asked one of the boys he knew at the *settlement* to have lunch with him, but the boy had refused and said, "I can't because *I ain't got none*." The other boys had lunch in the cafeteria and practised all their yells.

In the afternoon Lloyd and the rugby team from the collegiate went to the stadium for the game. There was a huge crowd and the cops were very busy directing the people. The spectators said the game was the best the boys had ever played and Lloyd, who played *half-back*, was star. Meanwhile Patricia had been a most effective *jumping centre*. All the *boarders* and *day-girls* went to Rosedale to see the game, which was very thrilling.

When Patricia and Lloyd came home that night they were very excited and told each other all the details of the game. When Mr Woodworth came home he was very pleased that both his children had played so well and enjoyed their sports so much.

(3) *Method*

The chain or serial reproduction method[1] with 2–8 chains in each group for each story was used. The subjects read the story through once and immediately wrote what they remembered of it. They then left the room and the subjects for the next link came in and each read one of the former subjects' reproductions. A week elapsed between each story. The chain was carried on until either the subjects or the material were all used.

III. RESULTS AND DISCUSSION

(1) *Change in proper names*

In story 1 there are four proper names and in story 3 three. Of these Mr Winkleyes and Mrs Doonought are unfamiliar and not found in the subjects' life. Mary-Ann, Joseph, Lloyd and Patricia are familiar and real to all the children, while Mr Woodworth is a possible real name for, though the actual form may not be known to, all the subjects.

The *qualitative* study of the forms the names take at the *end* of each chain shows that they are either as familiar and commonplace as the original or, in the case of the imaginative names, more so. For example, Mr Winkleyes at the end of various chains becomes Davies, Wimply, Anderson, Dinney, Winkle, Wilkins and 'Uncle Wigeley', while Mrs Doonought becomes Connought, Duggan, Dinna, Danby, Doughnut and Donald. The examination of the successive forms these names take *through* any one series shows this same tendency towards the familiar. Thus Mr Winkleyes successively becomes Winkeleyes, Wilkins, Wilkinson, Anderson.

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[1] Bartlett (1), p. 119.

There is complete disregard for sex. The characters change sexes frequently, becoming Mrs Winkleyes and Mr Doonought, and often taking on the much more conventional form of 'Mr and Mrs Winkleyes' or 'Mr and Mrs Doonought'.

It might be noted that in story 2, where no characters are mentioned by name, the public school 10-year-olds in two out of four cases designated them as 'the two Indians' and in the third case 'Jim and Tom'. This did not occur with other groups.

The *quantitative* study of the changes in names is shown in Table I. The horizontal columns designate the various school groups and their ages. The vertical columns show, first, the number of possible times a name could be given (number of chains × number of links) and under each name (*a*) the number of times it was given = No., (*b*) the number of changes which occurred = Ch., and (*c*) the percentage the number of changes is of the number of times the name is given = %.

Table I. *The number of changes occurring in proper names*

			Winkleyes			Doonought			Mary-Ann			Joseph		
School	Age	P.T.	No.	Ch.	%	No.	Ch.	%	No.	Ch.	%	No.	Ch.	%
Public	15	36	36	21	58	34	19	56	28	2	7	8	2	25
Private	15	36	36	22	63	26	12	65	36	1	3	21	0	0
Private	10	32	32	21	65	10	5	50	27	1	4	15	2	13
Public	10	36	30	13	43	6	6	100	21	3	14	11	2	18
Country	14	20	13	7	54	7	7	100	13	1	8	5	1	20
Total		160	147	84	—	83	49	—	125	8	—	60	7	—
% Total changes of total no.					57			60			6			12

			Patricia			Lloyd			Woodworth		
School	Age	P.T.	No.	Ch.	%	No.	Ch.	%	No.	Ch.	%
Private	15	40	40	5	12	39	3	8	32	6	19
Private	10	24	24	2	8	24	0	0	13	6	46
Public	10	32	25	4	16	23	4	17	15	9	60
Country	14	16	16	2	12	16	2	12	3	3	100
Total		112	105	13	—	102	9	—	63	24	—
% Total changes of total no.					12			9			38

From this table it is apparent that:

(*a*) Unfamiliar names change much more often than familiar ones. Mr Winkleyes and Mrs Doonought change 57 and 60 per cent. of the times they are given; Mr Woodworth 38 per cent.; and Mary-Ann, Joseph, Patricia and Lloyd, 6, 15, 12 and 9 per cent. respectively.

(*b*) The *total* number of times a name is given does not seem to be related to its familiarity but rather to its importance in the story; thus Mr Winkleyes and Mary-Ann are given 160 and 125 times respectively, but Mrs Doonought and Joseph only 83 and 60 times.

The *differences* among the groups is not marked, either in quality or quantity of changes. It may be noted, however, that the public school 10-year group made the highest percentage changes in four of the names, and on Mr Winkleyes, which was one of the highest in changes in total score, this group scored lowest.

(2) *Quantitative analysis of the recalled material*

The relative amount of the respective parts of each story was remembered by each group is shown in the following graphs. The graphs for the first two stories were obtained by dividing the story into major sections and noting how often something belonging to that section was recorded. The number of times the section was given by one chain was added to the number of times it was given by the other chains in the group. This sum is recorded as a percentage of the total times it could be given. The solid blocks show that the essential items of the section have been given; the lined blocks show that something changed from the original, or something arising out of it, but stated in different terms, has been recalled.

The graphs shown in Fig. 1 for story 1 show:

(*a*) There is a similarity in general contour. The section about the 'party', given in some form in nearly 100 per cent. of the papers, is in each case the most prominent item. The section about the beetle, or some sort of insect, is recalled in next greatest amount, and the sections following this show a gradual tapering off to the conclusion.

(*b*) The section about the 'conversation' varies in amount from group to group. The two groups from the private school retain it to a greater degree than the others, and on referring back to the papers it is found that it is the item about the spinach rather than the ice-cream that is retained.

(*c*) The difference in the amount the title is retained varies from school to school and is probably to be attributed directly to school training.

The graphs shown in Fig. 2 for story 2 show:

(*a*) The general contour is similar for each group. The introduction, which gives the general setting, namely, that there are people doing some sort of outdoor activity, is retained. The conclusion, which is certainly dramatic, stands out.

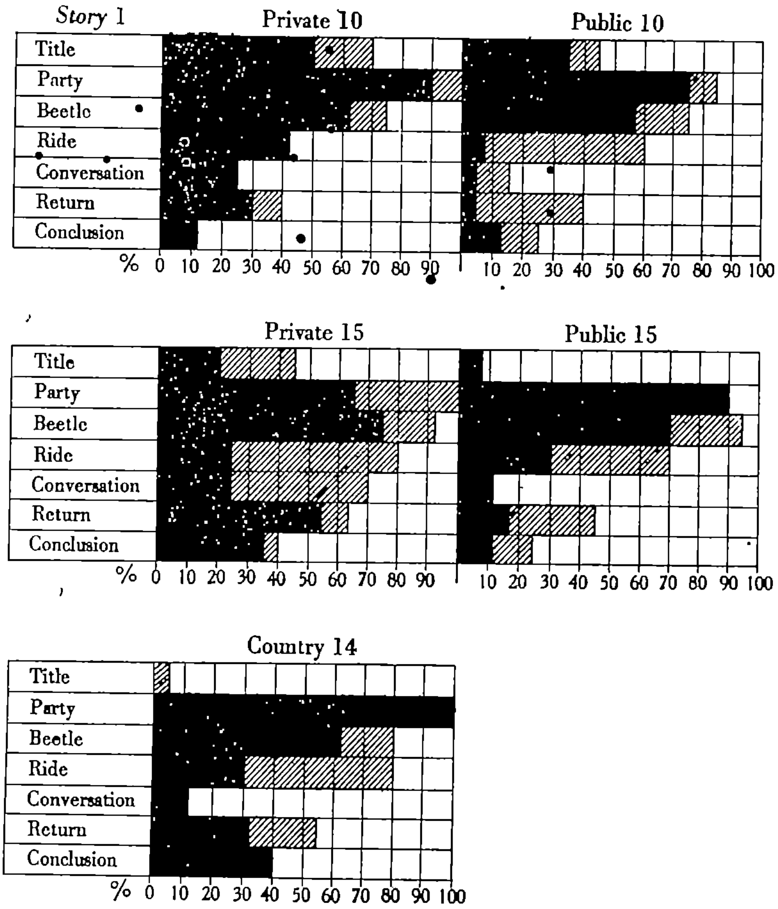

Fig. 1. Amount that each section of story 1 was remembered.

(*b*) The graphs differ in contour from those of story 1.

(*c*) The paragraph of the conversation which gave the reasons of the story, and were the only reasons or explanations given in it, is retained to some extent in all groups, and to 100 per cent. in one group.

Story 3 was studied in a rather different way. In it every phrase was scored in the same way as the paragraphs of the other stories. Then the key words, that is those having a high degree of meaning for at least one of the groups, were selected and the recall value for them is given on the graph shown in Fig. 3.

From this graph the following points arise:

(*a*) There is a great difference in the shape of the graphs given by the various groups. This contrasts with the other two stories, which were not 'weighted'.

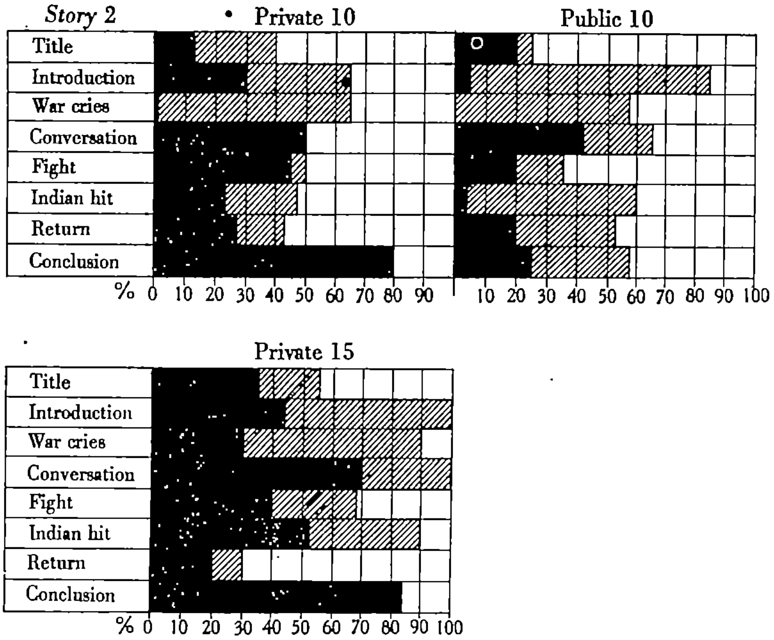

Fig. 2. Amount that each section of story 2 was remembered.

(*b*) The phrases which give the highest degree of recall differ for each group. Thus the highest value is given by the following groups for certain phrases:

Private 10. Basketball, order mark, jumping centre, catechism.

Public 10. Basketball, rugby, sewing.

Private 15. Rugby, basketball, VM science, order mark, jumping centre.

Country 14. Rugby, P.T., manual training.

These phrases all have a high degree of meaningfulness and familiarity in the social setting of each school. The private school's chief game is basketball, and order marks are an essential but disagreeable part of the social structure. The younger children at the private school learn the catechism, and the older ones find the rugby games which the boys' schools play a vital part of their social life. At the boys' school rugby is probably the chief item of importance to the boys and it is recalled 100 per cent. Thus the graph would tend to suggest that items having a high positive value of social meaningfulness are remembered to the greatest amount.

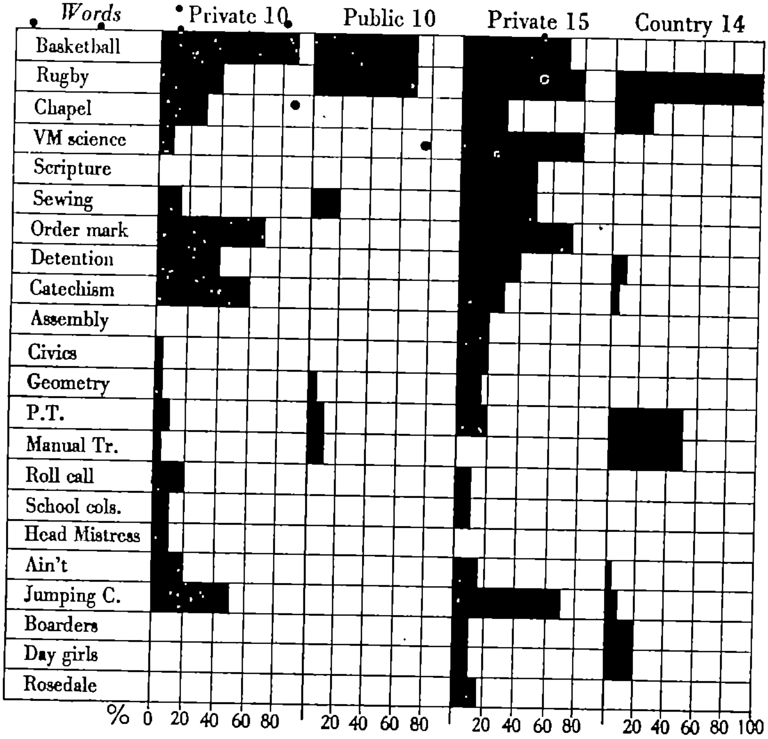

Fig. 3. Amount that the key words in story 3 were remembered by the various groups.

(c) A small point which suggests a contradiction to the above statement is the retention of the phrase 'ain't got none'. This is a common phrase but discouraged by the school authorities for the children in the public school. They are the only ones who failed to give it at all. The other schools, in which it would never form part of the children's vocabulary, all give it to a slight degree. This may suggest that those things which are very foreign to the culture of the group assume a positive value in their 'startling effect' and thus are retained. This is a suggestion which could be experimentally tested.

The *relationship* between the changed and unchanged material is shown in Table II. This gives the changed material (*i.e.* that recorded in lined blocks) as a percentage of the unchanged material (that recorded in solid blocks) for the same group.

Table II

	Private 10	Public 10	Private 15	Public 15	Country 14
Story 1	19	80	65	48	33
Story 2	69	230	85	—	—

Comparing the stories it is seen that there is more relative change in the second story. This would be subjectively more difficult or 'less meaningful' than story 1, especially for the younger children. It is probable that its difficulty for the older children would not be much greater. This suggests that the 'less meaningful the material is for the subject the more he tries to recast it in some more meaningful form'. This point is merely suggested and its investigation is being carried on at the present.

(3) *Recasting of material*

By 'recasting the material' is meant that the story is given a different form, setting, or style, or that the statements are so varied, distorted or elaborated that the meaning becomes quite foreign to the original.

The *qualitative* study of the recasting reveals points which are given under the various stories.

(*a*) In story 1 the usual form given consisted in loss of the fairy element and ride to the clouds, and the retention of something about a party, a ride and a beetle, but in a very matter-of-fact style.

(*Private school,* 10 *years, link* 2.) Mary-Ann went to see Mrs Doonot and Mr Winkeses and to have tea there. "Would you like to see my beetle?" asked Mr Winkeses. "Yes indeed", said Mary-Ann. "Come then and I will show you them", said Mr Winkeses. The beetle winked at Mary-Ann and after a while Mary-Ann said, "I think we had better go because its nearly supper time."

(*b*) Story 2 usually lost the ghost element, and was turned into a tale of two men doing something outdoors, being asked to go to war, making excuses, but finally going and being killed.

(*Private school,* 15 *years.*) Two young men were paddling up a river in a canoe. Some were on the shore at a picnic. The two young men called, saying "We are going to fight the Indians, who would like to come?" One of the picnic party said, "I might get killed and my relatives would not know where to find me." So only one man went to fight the Indians and the other went home. Suddenly a big black thing fell out of his nose and he fell on the ground dead.

(*Public school,* 10 *years, link* 3.) One day as two young men from Eugal went out fishing they heard a voice coming from the wood. One man said, "Come we will see what it is." There they saw two men by a stream of water screaming. They asked them what was wrong, they told them a beaver bit them.

(*c*) Story 3 was recast less frequently than the other two. It was more usual for parts to be selected, and what recasting there was was achieved by merging the two games into one. The younger children, especially of the public school, did, however, make some real recasts.

(*Public school,* 10 *years, link* 2.) The children of two schools were having a final game. Mr Woodmore was just about to ask his daughter to join the team, when the children of the two schools began to play. Mr Woodmore's daughter was just in time to play.

Table III shows the *quantitative* results of the recasting. This shows for each group the average link in the series at which the recasting took place.

Table III. *Link where recasting occurred*

School	1	2	3
Public 10	2·5±0·7	2·5±0·5	1·5±0·5 (2 no recasts)
Private 10	2·5±0·5	3·0±0·5	4·0±0·5 (1 no recast)
Country 14	3·0±0·3	—	7·0±0·0 (1 no recast)
Public 15	4·0±0·5	—	—
Private 15	4·5±0·5	5·0±0·7	No recast

From this table it is shown that the link in which the older subjects recast the story is further along in the series than that of the younger ones. It seems this is not related to the total amount of material remaining, but this has not been quantitatively shown. It probably shows what was noted above—that recasting is a more gradual process with the older subjects, who take two or three links to incorporate the modifications after they begin to appear.

The following are *some comments about the recasting*:

(*a*) *Form the recast material is given.* Usually one of two things happens in the recast material. (i) The essential situation and broad outline of the story is preserved, that is, in story 1 the fact is given that there was a social situation to which people went, did something there and left, but the details of the proceedings are varied. Or (ii) some one

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detail is selected and elaborated into quite a different setting. For example, in story 2 the death is selected and a tale constructed of quite another sort of death. The first of these two types seems the more common, especially with the older children.

(*b*) *Age differences*. The younger children differ from the older in the following ways. (i) Their recasts show more diversity as they occur in various chains for any one story. (ii) They change the story more abruptly. The whole recast is apt to appear suddenly, whereas with the older children it gradually creeps in through several links. (iii) They recast the story earlier in the series than the older ones do. This lends support to Blatz's (2) suggestion that children are interested in a concrete world and imagination is a developed ability. In this case the older subjects seem to be able to retain imaginative settings and not bring them down to concrete forms for a longer time than the smaller children.

(*c*) *Social differences.* Of the 10-year-old groups the children at the public school (i) recast the story earlier than the children at the private school, and (ii) were much more apt to elaborate details and to give stories showing much more diversity of form.

(*d*) *Permanency of recast.* Once the story has been recast it tends to remain without further major change to the end of the series. Whether this is because it has reached a form favourable to the group, or whether it is because in most cases it has been reduced to its bare essentials is an open question. There are exceptions to this permanence; in each of the two younger groups a second recasting in two of the chains of story 1 is given, and the public school gives one second recasting on story 2. These second recasts of the story seem to involve additions.

(*e*) *Story* 3. As has been indicated, story 3 usually, with the older groups, tends to selection rather than any real recasting. The younger groups, however, and especially the public school children, do construct some real recasting.

(4) *Additions*

The distinction between addition and modification is based on subjective judgment and is often difficult to make. Any material which does not appear to be derived from the original is treated as an addition.

A qualitative consideration of the additions reveals the difficulty of classifying them into types. Those given by the younger children are especially varied. Those of the older children tend frequently to be a sort of enlargement of the original rather than a piece added to it. A few of the additions are given below:

Story 1. They went to the party in Mr Winkleyes car. Mr Winkleyes cooked dinner. It grew cloudy and the children were afraid. He took Billy and Mike to Sunnyside.

Story 2. One man was sitting at the stern, the other at the bow. They saw all kinds of weird shapes. They took the men to a hospital.

Story 3. Mr Rubens presented the team with a cup. They lifted Tom on their hands and were very happy. Tom became a star and played for the biggest team in town. Father gave them a present for it was a day off from work. Patricia walked home with one of the players. They won.

It may be noted that most of the additions consist of a phrase very familiar to the social groups which gave them. For instance, one could guess which group gave 'a day off from work'. Sunnyside is a popular amusement park, and bows and sterns of canoes are well-known terms to at least one class of Canadian.

The addition about Tom's ovation and the frequent additions which were given saying the team 'won' are interesting. The story did not end in that fashion, but according to all precepts about popular stories it should have done so. There seems to be a certain idiosyncrasy in human nature which demands a successful or at least a complete ending to round off the tale. This is first seen probably in the nursery tales which end 'so they lived happily ever after'.

Quantitatively we may study the additions from Table IV. This lists (*a*) the average number of additions made by each group for each story, (*b*) the average link of each chain on which the additions occur, and (*c*) the average number of links through which the additions were retained.

Table IV. *Additions*

		School					
	Story	Private 15	Public 15	Private 10	Public 10	Country 14	Average
Number	1	5	1	8	10	1	5·0
	2	4	—	4	12	—	6·6
	3	5	—	4	17	2	7·0
Occurs	1	4·0 (10)*	3·0 (9)	2·8 (4)	4·2 (10)	3·0 (3)	
	2	4·5 (10)	—	2·3 (8)	4·3 (10)	—	
	3	4·0 (10)	—	2·0 (6)	2·8 (4)	4·5	
Duration	1	5·0	1·0	1·6	2·5	1·0	
	2	4·5	—	4·0	4·0	—	
	3	3·8	—	4·8	2·5	2·0	

* Numbers in brackets show total number of links in chains of that group.

From this table it is seen that (*a*) the *Number* of additions is between five and seven for each group, and as there are usually four chains in each group this means that only one or two additions are made on an average in one chain. The public school juniors make considerably more additions than the others, their total on the three stories being 39 and the comparable private school group 16. The younger groups on the whole make more additions than the older ones.

(*b*) The *occurrence* of additions shows high variability, but the above table gives the average position. When this is taken with the number of links in the chain it is seen that additions tend to occur just before the chain is half complete. As the chains were usually continued till the material was reduced to a minimum, the place where the additions occur may be related to the amount of material present.

When the position of the additions is compared with the place at which recasting occurs, as shown in Table III, it is seen that in each case the additions occur just before the recasting takes place in each story with each group, except with the public school 10-year-olds, and here in each story the recasting definitely precedes the additions. The significance of this is a matter for investigation.

(*c*) When the average *duration* of an addition is related to the average link on which it occurred and this is compared with the total links of the chain, it is shown that additions once made tend to remain throughout the series. As the variability of both these averages is high this must be taken as extremely tentative, but from the qualitative study in which it was shown that additions were usually of something highly familiar and meaningful to the group which made them, the retention of them by the group seems quite natural.

(5) *Modifications*

By modification is meant a change in a phrase or a sentence in which the relation to the original seems clear. The term 'modification' serves to cover several types of items which were found in the material and which are given under the following sections:

(*a*) A word or sentence gives rise to a *Conventionalization*, which means that it is put into current phraseology or given a meaning which is common to the group.

"Come up and see me sometime." (Story 1.)
Because I have to ask Mother if I can go. (Story 2.)
He died of fright. (Story 2.)
The team Patricia was 'sticking for'. (Story 3.)
Her 'girl friend'. (Story 3.)

(*b*) A *rationalization* may be introduced to make the story more reasonable and coherent.

Horses were eating the spinach. (Story 1.)
They went to *see* the pink clouds. (Story 1.)

(*c*) There are direct *reversals* in the material; in these the phrases and sentences are turned into the direct opposite of the original.

> He said that he liked spinach. (Story 1.)
> He had a wife and family at home so he said he would go. (Story 2.)

(*d*) *Substitutions* occur in which the original reason or activity is changed into another reason or activity of quite a different sort.

> Mary-Ann shall go first because she behaved best. (Story 1.)
> They ran races. (Story 3.)

A quantitative analysis of the data shows that while all groups give instances of those items which have been considered conventionalizations, the items classed as reversals, rationalizations and substitutions are found only in the older groups. As these occur rather infrequently, even with the older children, this may be of no significance.

The modifications are always towards a more real and conventional setting, and tend to mould the material into something more familiar to the groups.

(6) *Material retained throughout the series*

A list of all the material from the final links of the chains reveals (*a*) certain qualitative characteristics of the persisting material, and (*b*) differences in the material kept by various groups.

(*a*) The material retained through the series shows much the same characteristics as that considered under 'recasting'.

(1) The general outlines and main features may be retained. Thus in story 1 the total setting of the party is kept:

> A little girl was at the party at Mr Winkles.

(2) Some detail which seems rather useless to the story as a whole may be selected and retained:

> Mr Doonot and Dinna were eating cake with pink balls.
> Mr Doonot asked, "Do you like spinach?"

Or (3) something which has been developed or inserted may be retained to the end. In story 3 the winning of the game remained in six of the final links.

These three types of results may be significant; they correspond with types common in 'everyday' remembering. Thus one may recall the general thesis of a speaker or one may only recall that he had a very heavy watch chain. And corresponding to type 3 one knows that when one reproduces a story one frequently inserts items such as the 'winning' in story 3, which round the story off or make it a good tale. From the data there is no indication of why or when the respective types arise, except as shown in certain cases discussed in (*b*).

(*b*) There are differences among the groups. The older children keep the general outline for all the stories more than the younger ones. However, it is only in story 1 that the second type—insignificant details—are retained throughout the chain, and these are always given by the older children.

Invented material is given predominantly by the younger children and more especially by the public school ones, who give a good deal of it and show great diversity in the various chains of the matter invented. It may be restated that invented material does not mean imaginative material, in the ordinary sense of the word. It rather throws the material into a more usual and probably more satisfactory form. This may suggest that older children have acquired the ability to retain objective material as such to a greater degree than younger ones who must relate it to themselves and cast it into familiar forms; or it may suggest that as invention occurred mostly in stories which would be difficult or unfamiliar for the reader, there is a tendency to transform difficult and unfamiliar material into something more familiar and probably more pleasing. This difference in type of material recalled is worth further investigation and could probably be attacked better by repeated reproductions of one person than by the serial method.

(7) *Ideas dominant at the end, and the course of their development*

From the material given at the end the idea which seems to be dominant is selected. Adding the dominant ideas from the various chains Table V is obtained.

Table V. *The dominant ideas in the final link of the series*

Story	1	Story	2	Story	3
Party	16	Fight	8	Games	15
Beetle	5	Indians	3	Captain	1
Ride	3	Fishing	1		

From this table it is clear that the ideas which become dominant do so not from their position in the story—the 'fight' being near the middle, and the party and games near the beginning—but rather because they are what may be designated the 'active centres' of the story.

A table has been made, but is not given here, which traces the idea dominant at the end of every chain back through each link of the chain to the original material. This records the position which the idea dominant at the end has taken in each link. This shows that the position of the dominant idea varies considerably in the different chains, and in each comparable link. So it would seem that an idea becomes dominant not because of its position, or of any other 'mechanical' factor, such as its frequency or recency, but rather because it is the centre of a system of activity and controls it. The final system of activity given—such as 'going fishing' in story 3—may not be the same as the original one, which in this case was the 'war'. This is an exception, however, and when it does occur is usually found in the younger children, especially the public school group. These cases, when a substitute centre is given to the story, are probably of special interest, and may be related to the unfamiliarity of the material, or more probably to the individuality of one member of a chain. The 'weighting' factor of one person who may deflect the whole story is a point which so far has not been investigated. It might possibly be used to gain insight into the mental make-up of that person, and may have certain clinical value.

(8) *Style*

Style is the most difficult aspect of the material to study experimentally. But in general the reproductions were given in short, rather jerky, sentences, and especially with the younger children in the type common to children's essays and 'compositions'. The sentences rather than the paragraphs became the units of thought, and the paragraphs dropped out both in form and in content.

IV. CONCLUSIONS

(1) *Specific*

(1) *Proper names* are changed to a form as common or more common than that given in the material. The changes are more numerous for unfamiliar names than for familiar ones.

(2) The outline profile of the *amount* each part of the material was remembered shows similarity among the groups for one story, but differences between the stories. The part remembered most was that which was the centre of the activity or the social setting of the story.

When the phrases were 'weighted' the profile for each group was different and showed that each group selected those items which were familiar and meaningful in its social background.

(3). The *recast* of the story either contained the general outline or elaborated some detail of the original and took a form which was highly meaningful to the group. Once it was recast it usually retained its new form throughout the series.

The younger subjects recast the story earlier in the series than the older and gave more diversity of form in their various recasts. The public school children showed both these characteristics to a greater extent than the private school children.

(4) The *additions* made were highly meaningful to the group giving them and once given they tended to be retained. Those which brought the tale to a happy and satisfactory ending were frequent.

The additions for most children occurred before the recasting of the material took place. The public school children did the exact opposite and their additions were made after they had recast the material.

(5) *Modifications* tended to bring common idioms into the story, and to twist arrangement of words and phrases in order to give a more usual and matter-of-fact account. Direct reversal of statement also occurred.

(6) The *material which remained through the series* either retained the general outline, elaborated some detail, or developed some item which had been inserted in the story. The older groups showed the first two of these, while the younger public school group gave most invented material.

(7) The *ideas which were dominant at the end* were those which may be called the 'active centres' of the story. Their dominance seemed to depend on ideational construction of the story rather than on any mechanical factor such as position. The younger public school groups sometimes created other active centres, which had some nearer interest for them than the original.

(8) The *style* turned into short, rather jerky sentences which are similar to the form children give in their school 'compositions'.

(2) *General*

Children's remembering seems to reveal the same characteristics as those shown in Bartlett's work with adults. It shows tendencies to cast the material in familiar terms, phraseology and social settings, and to omit unusual elements and any 'style' the material may possess. By changes, additions and omissions the material is always being moulded into a form which is usual and familiar to the group.

There is some indication of *age* difference; the older children retain the given material in its own form more accurately and for a longer time than the younger ones, who almost immediately by omission and addition cast the material into a form nearer their own interests. It seems that this type of experiment should be done with much younger children to see what light genetic study would shed on the process.

One difference among the *social* groups is shown repeatedly; that is that the younger public school children show much more invention, far less accuracy, and much more diversity within the group than the groups in more stabilized settings. By consideration of the heterogeneous cultures from which the public school population comes, the diversity of response within this group, and the difference it showed from other groups, the theory that remembering is moulded by the background, *mores* and patterns of the social group is not only supported but shown to be true even when the social groups belong to almost identical geographical environments.

However, these results may be generalized in another way, namely the more 'unfamiliar', 'meaningless' or 'difficult' the material is for a subject (or the more foreign it is to what Lewin calls the 'psychological field' (3)) the more he tends to substitute a story of his own invention, stated in terms familiar to him, rather than to recall that which is originally given. The material may be 'afar' from him because it belongs to a different social setting, or because he does not understand it on account of its 'difficulty', but whatever the cause, the more foreign he finds it the more he changes and re-forms it into words and sentences familiar to him, and to the social group to which he belongs. It seems, therefore, that the problem of remembering is one of 'construction'; belonging both to individual and social psychology, it lies on the border line where the two meet.

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REMEMBERING IN DIFFERENT SOCIAL GROUPS

By R. S. MAXWELL

I. *Aim of the experiments* (pp. 30–31).
II. *The groups of subjects* (pp. 31–32).
III. *Results of the experiments* (pp. 32–40).
  (*a*) *The treatment of mistakes* (pp. 33–34).
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    (i) *Shortening* (pp. 37–38).
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IV. *Oral reproductions* (p. 40).

I. Aim of the experiments

In his discussion of the social determination of remembering Prof. Bartlett remarks: "the specific bias, appetitive, instinctive, ideal, or whatever else it is, in the group, awakens in the individual too an active tendency to notice, retain and construct specifically along certain directions."[1] The main aim of the experiments herein described was to obtain further evidence of these social factors in recall, in so far as it is possible to do so by the use of the method called by Bartlett the method of Serial Reproduction.

I constructed an original story as follows:

> It was half past seven on a fine July evening when Father Malone knocked at the front door of Sir John Tribe's London house. He had hurried round from the Church where he had been saying Mass, so that he would not be late for dinner.
>
> Sir John had only just arrived before him, and was resting in an armchair near the large open window in the lounge. He was very tired, because he had been playing cricket for the M.C.C. in an important match, and had had to bowl continuously at both ends for an hour and a half without any break.
>
> After a minute or so, Mrs Tribe, his wife, came downstairs, accompanied by Miss Weaver, a friend who was staying in the house, and switched on all the electric lights.
>
> Then they all went in to dinner, and the butler at once served them with apple pie.
>
> About ten minutes later, when they were all talking and laughing quite cheerfully, Miss Weaver suddenly gave a terrific shriek and fell forward across the table with a bullet wound through her head. When the others reached her, they found that she was dead, and Sir John at once plucked the blood stained knife from her heart.

This story contains five different points which it was thought might appeal to the socially determined interests of different groups of people.

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[1] F. C. Bartlett, *Remembering*, p. 254, Cambridge University Press, 1932.

At each point one or more mistakes were deliberately introduced. The story was prepared before the writer had seen any of Bartlett's results, and it was anticipated that certain of the errors would be particularly outstanding for certain of the subjects, and thus would persist throughout. If such a tendency existed, however, it was completely counteracted by the process of rationalization,[1] and almost every group altered the errors and produced what individual subjects described as a 'better story', or 'a good story', or 'a smooth story' before the end.

The six mistakes were:

1. *Religious*: Mass is said in the morning, and may not be said in the evening.

2. *Sporting*: No bowler in cricket can bowl two overs consecutively, and consecutive overs are bowled from opposite ends of the wicket.

3. *Society*: Sir John Tribe's wife would be called Lady Tribe, not Mrs Tribe.

4. *Common Sense*: Electric lights would not normally be needed at 7.30 on a fine July evening if there were a large open window in the lounge.

5. *Domestic*: It is unusual to commence a formal dinner party with apple pie.

6. *Blatantly Obvious*: If Miss Weaver had been shot through the head it is unlikely that Sir John would have had to remove a knife from her heart.

For any one group the method of procedure was the following: Subject 1 read the original story through twice slowly. After a quarter of an hour, during which his attention was distracted, he reproduced as much as he could remember. In the case of adults, the subject wrote out his version himself. In the case of children (*i.e.* groups H, J and K) the version was dictated to the investigator. Subject 2 then read Reproduction 1 twice, and after a quarter of an hour effected Reproduction 2. The process was continued until all subjects in a given group had been tested.

II. THE GROUPS OF SUBJECTS

Twelve groups of varying age and social conditions were tested, ten male and two female.

Males

A. *Educated adults.* This group consisted of professional men, doctors, schoolmasters, university lecturers and professors. All the subjects were university graduates, 75 per cent. of them had obtained doctorates, and two were Fellows of the Royal Society. Average age—45 years (approx.).

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[1] Bartlett, *op. cit.* 126–7, 136–8.

B. *Priests*. The majority of these held a university degree. Average age—45 years (approx.).

C. *Adults* attached to All Saints' Church, Notting Hill, W. 11. Most of these were engaged in clerical work in the City although one or two were manual workers. Average age—30 years (approx.).

D. *Students of Physics* at St Andrews University. Scottish undergraduates in their third and fourth years. Average age—21 years (approx.).

E. *Students* at Westminster Training College, London, S.W. 1. These men were training to be teachers and came from all parts of the country: most of them were Methodists. Average age—20 years (approx.).

F. *Soldiers*. The official title of this group is "Recruits for the Combatant Mechanized Branch of the Army". The subjects were recruits for the Royal Tank Corps; they came from all parts of the country and were tested at Bovington Camp, Dorset. Six groups of recruits were tested, and these are designated Groups F 1, F 2...F 6. Average age—20 years 5 months.

G. *Senior boys* at King Edward VI School, Nuneaton, Warwickshire. Average age—17 years 3 months. This is one of the old grammar schools, and is now co-educational.

H. *Junior boys* at Manor House School, Clapham Common, S.W. 4. Average age—13 years 10 months. This is an independent secondary school: the boys are of good social position.

J. *Boy Scouts* attached to All Saints' Church, Notting Hill, W. 11. Average age—14 years 0 months. All the boys came from working-class homes in a poor district of West London. Most of them were still at school, but a few were in manual employment.

K. *Boy Scouts* attached to All Saints' Church, St Andrews. Average age—13 years 6 months. This group consisted of Scottish boys from working-class homes. Most of them were still at school, but a few were errand boys.

Females

V. *Educated adults*. These ladies were mostly the wives or relations of the subjects of Group A.

W. *Students of Physics* at St Andrews University. The female counterpart of Group D. Average age—21 years (approx.).

III. RESULTS OF THE EXPERIMENTS

Since the main interest of the present investigation was to obtain evidence for socially determined changes, no attempt will be made to analyse in detail the changes introduced in a chain of reproductions as a result mainly of individual idiosyncrasies. But in view of the fact that Bartlett's results were not available when this experiment was in progress, it is worth while pointing out that exactly parallel changes were undergone by his stories and by mine.

(a) *The treatment of mistakes*

Nearly always the awkward points of my story were speedily smoothed out and the mistakes corrected. This was rarely or never done wittingly. If a mistake reappeared in any version and the subject was questioned, he would say: "Oh, I really am not absolutely certain. It couldn't have been *that*; I must have remembered it wrongly." Several subjects made remarks such as: "It seems a better story now; I can picture it better"; or, "It is smoother now", although they assured me that they had faithfully tried to reproduce it accurately.

As regards the saying of Mass at 7.30 p.m., the words "in the evening" were frequently omitted. Twice "early morning Mass" or "early Mass" was written. Once the priest "had been playing cricket all the previous afternoon"; another time he came home "at the end of the morning", and the next subject added the information that "it was Sunday". As will appear later the reference to Mass persisted throughout in four series only, and in none of these is any time specified. Two of these were boys' series, and two were obtained from women. In one set "Low Mass" appeared in the fifth reproduction, and thereafter persisted throughout. The term is very familiar to all subjects in this group, most of whom are altar-servers.

The mention of cricket persisted in the two student groups, in three of the soldier groups, and in one group of adult women. But the bowler, as a rule, very quickly ceased to operate at both ends, and was merely said to have "done a lot of bowling", or to have "bowled for two hours", or "for most of the game". With the men's series the letters M.C.C. persisted for some time, but were changed sometimes to "Lord's", and in one case to the "Oval". The Scottish Boy Scouts did not know what they meant, and first altered them to M.M.C. These letters were equally meaningless and speedily disappeared altogether. In a women's group which later gave oral reports of the story, the whole point was altered to "a strenuous day's bowls on the green of the L.C.C."

The electric-light incident was dropped out of all series.

Apple pie remained to the end in two cases only: one of the soldier groups, and the women from St Andrews. The alterations which this incident suffered are of some interest. In one case a change took place immediately into "as the butler was serving the soup". Or again all mention of dinner was dropped and the report was "the butler served some apple pies". The Scottish Boy Scouts kept the pie, but dropped the butler: "Miss Weaver came downstairs with the apple pie." Several times the apple pie ceased to head the *menu*, but appeared when the meal has been in progress for some time. Once it simply changed to "the pudding", and once, dinner having been replaced by tea, it appeared as "apple cake".

The episode concerning the bullet and the knife persisted well, appearing in some form or another in every final record except the one from the adult women. "Knife" was more often than not changed to "dagger", and sometimes either the bullet or the knife was omitted. Once a "doctor" produced or found a dagger independently and without reference to any shot. And again, as an entirely independent item of information, it was said that "Mr Willmer has a sister who had been shot." One way and another it is clear enough that this kind of material produced a considerable impression within every group of subjects. It is of the type familiar in the more sensational Press and in much current popular literature, and may, in addition, of course, have some direct emotional significance.

In six of the fifteen series Mrs Tribe was altered to Lady Tribe (or whatever proper name was being used). Once only Sir John becomes Mr Tribe. Once "Lord —" appears as the husband of "Her Ladyship". Whether or not this is any reflection of the alleged bias on the part of popular groups for titles is not particularly easy to decide.

The bulk of these processes of change applied to the original mistakes have the general nature ascribed by Bartlett to rationalization.

(*b*) *Inventions and additions*

As always, proper names were the most variable of all the elements. Sometimes they were altered immediately to forms which had no apparent relation to their originals, and sometimes a regular sequence of forms resulting in some final acceptable name took place. A good example of the latter is: Tribe—Traube—Frauke—Franke—Franch—French. Probably, under the conditions of these experiments, some part is played in such a sequence of change by bad or indistinct writing. Once Sir John French had appeared, the name stuck.

The interchangeability of certain terms was constantly illustrated. This occurred, for example, with knife—dagger, and with heart—breast. Words such as these alternate frequently throughout every series, a fact which may be traced to the effect of common and conventional phrases.

Some of the inventions, as in Bartlett's cases, appeared to be definitely individual, and to be connected with visualization. These always had the effect of making the story more vivid and pictorial. Subject E 9, for instance, wrote the following version:

> Dr Munroe returned at 6.30 from celebrating Mass. Shortly afterwards Mr Tride came in, very tired, having been bowling all day, from both ends, for the M.C.C. They each took a chair by the window and were soon joined by Miss Weaver and Mrs Tride. The butler brought in the apple pie and they were about to commence when Miss Weaver collapsed dead: killed by a bullet fired through the open window.
>
> Dr Munroe leaned forward and pulled out the blood-stained knife.

E 10 visualized all the incidents strongly, and now several interesting elaborations took place.

> Dr Munroe returned home from Mass at 6.30. In half an hour he was joined by Dr Bowls who had, for the greater part of the day been bowling from both ends for the M.C.C. Before long the two gentlemen were joined by Miss Weaver and Mrs Bowls.
>
> Suddenly, the conversation was interrupted by a loud report, and Miss Weaver, with a gasping, half-articulate cry, collapsed in an inert heap. The two men realized that she had been killed by a revolver shot fired through the open window.
>
> The doctor, with a dramatic gesture, drew out the blood-stained dagger.

Here it will be seen that considerable elaboration has been produced although at the expense of the butler and the apple pie.

The additions which are inserted unwittingly, with the general effect of explaining the actions of various characters in the story, are particularly interesting.

In group C, the adults attached to All Saints' Church, Notting Hill, came the comforting remarks: "No blame attached to anyone", and "It was a very hot day and it was nobody's fault at all."

One of the soldier groups explained that "Mrs Malone and her daughter had remained in the house *to get the tea ready*."

The boys at the Nuneaton school reported that "Father Rowley ran across to her immediately and pulled the knife out, *but it was of no avail*. She was dead."

Into the Scottish Boy Scouts' series came the explanation: "Miss Weaver *came downstairs with the pudding and put it on the table*."

In the group of educated women came the remarks: "Sir John *hurried upstairs and changed for dinner*"; and "They met Miss Fowler *whom they asked to lunch*."

Practically none of these introductions persisted for long. Yet it will be seen that each one of them is particularly in line with the social background of the group in whose reports it appears.

(c) *Final reproductions*

It is interesting to compare the final forms of the story, as obtained from the various groups. They are therefore given here in full. The number in brackets after each final reproduction indicates the number of subjects in that particular group.

Group A. The Canon accompanied Miss Driver back. A shot was heard as the butler was serving the soup at dinner. The canon leaned forward and withdrew a dagger which had been plunged in Miss Driver's heart. [14]

Group B. A business man, who had returned home from the city, was having dinner with his niece and two sisters.

Suddenly one of his sisters cried out "I've been shot" and fell forward. It was found that she had been stabbed in the back. [10]

Group C. The Vicar came down to the dining room, feeling very tired, after a game of bowls. The woman came into the room, a shot rang out. The dagger under her cloak. [10]

Group D. Lady French was tired, John French was tired too. He had had a keen day's bowling at Lords. Lady French and Miss Shepherd sat down. Somebody stuck a dagger into her. [15]

Group E. Mr Willmer met Mr Bowles who was bowling for M.C.C. Mr Willmer had a sister who was shot. They found the dagger. [29]

Group F 1. Lord —— was at the Church giving a sermon. Lady —— was also at the Church, listening to the sermon. [8]

Group F 2. Mr Malone was bowler at a cricket match.

While Mrs Malone and his daughter were preparing the tea, Mrs Malone was shot by a bullet through the breast. [8]

Group F 3. One fine evening Father Malone went over to Sir John Levitt's house, who was resting after a hard day's cricket.

The dinner was served, the first course was apple pie.

Miss Weaver dropped dead across the table with a bullet in her head, immediately after Sir John and Father Malone came downstairs. [6]

Group F 4. It was about 7.30 in the evening when Sir John Malone returned home after a hard days cricket.

He had only just arrived home when Miss Weever, and her sister arrived, just then the butler came in and said dinner is served, and Miss Weever dropped dead across the table. [4]

Group F 5. Approximately after half-past seven one July evening Father Malone returned from Mass both cold and hungry.

He was met by Sir John Simn's at whose house he was staying. Later in the evening Lady Simns accompanied by a young lady came down stairs to dinner.

Dinner was served by the butler, the first course being apple pie.

The young lady who accompanied Lady Simns fell to the ground with a bullet wound through her head.

Sir John Simns who reached the lady first plucked a blood-stained dagger from her heart. [4]

Group F 6. It was a fine July evening when Father Malone returned from the field where he had had a hard day's cricket at both ends. They found Miss Weever alone and rushed forward and drew the blood-stained dagger from her breast. [7]

Group G. Father Rowley went down to find his friend. He found him seated in a chair reading his newspaper.

After dinner he found his wife, shot through the heart. He pulled out the knife, but it was useless.

She was quite dead. [13]

Group H. Father Wilson said Mass—Mrs Hyse murdered in her room—bloody knife. [10]

Group J. Mr Pollard went to Low Mass. Mr Pollard was killed. [10]

Group K. Father Monn a lady fell with a knife in her back. [12]

Group V. Mr walked across the field. He had been at Mass.

Mr Blake had also been to early Mass. He had also been playing in a cricket match.

Mrs Arnot fell across a table. [15]

Group W. —— was invited to dine at Sir John Frike's London house at 7.30. He had been attending Mass and when he arrived, Sir John was gazing out of the window. Lady Frike and Miss Weaver came down and lunch was served. The butler served apple pie. A shot rang out and Miss Weaver fell, shot through the head. Sir John jumped up and, stooping down, pulled a dagger out of her back. [10]

These final forms differ so much among themselves that it is difficult to believe that they all derive from precisely the same original.

Their character will be briefly compared under the heads of (i) shortening, and (ii) surviving content.

(i) *Shortening.*

It is a regular feature of all reports obtained in the manner used in this investigation that considerable shortening takes place. The relative amounts reproduced in the various groups may, however, be of some interest.

If the amount retained is calculated in words we get the results shown in Table I.

It has not been considered worth while to give a detailed classification in terms of number of 'points' or 'incidents' reproduced. If this were done, in fact, it would be found to follow very closely the classification by number of words.

It will be seen that the adolescent student subjects have retained more than the adults, and that these, in turn, have retained considerably more than the small boys. This may well be, at least in part, a reflection of real group differences. The adults were on the whole inclined to regard the story as rather nonsensical, the students to treat it as entertaining, and most of the small boys as a task, and not a very interesting one at that.

Both groups of women remembered more than their corresponding groups of men, but the data here are too restricted to admit of any certain conclusions.

In general it is suggested that social factors, taken in conjunction with the type of material presented, may have not a little to do with the amount preserved in Serial Recall, but further investigation is needed.

Table I

Number of words in original story: 209

Group	Description	No. of words in reproduction 5	No. of words in reproduction 10
A	Adult males	54	38
B	Priests	46	43
C	Males at Notting Hill	53	31
D	Males at St Andrews	85	55
E	Males at Westminster	108	111
F 1	Soldiers	70	—
F 2	Soldiers	57	—
F 3	Soldiers	72	—
F 6	Soldiers	41	—
G	Boys at Nuneaton	75	49
H	Boys at Clapham	42	12
J	Scouts at Notting Hill	33	10
K	Scouts at St Andrews	33	11
V	Adult females	83	41
W	Females at St Andrews	82	72

(ii) *Surviving content.*

It would look as if a study of *what* survives should be of greater interest for our main problem than a study of the amount surviving. For abstractly everything in the material presented has to run the risk of being dropped out or transformed as a result of individual idiosyncrasy as well as because it does not fit well into a social background. And whatever incidents pass on from person to person, run this risk as many times over as there are subjects. Consequently what remains in the end should be acceptable throughout the whole group of subjects concerned, and if there are important social differences between group and group, these ought to be reflected in the final surviving content. It is perhaps unfortunate that I devised my story more with a view to seeing what would happen as regards the errors introduced than with a view to bringing out this influence of group differences. Better material could doubtless be used. Still, an analysis of points surviving is of interest.

If we omit words or phrases which survive in one group only, the number of brackets in the following list shows the frequency of occurrence of survival in the fifteen chains obtained, the letters indicating the group concerned. (Groups F 4 and F 5 are omitted because they gave very short series only.)

Knife (10). A, B, C, D, E, F 6, G, H, K, W. (In seven cases the 'knife' was changed to 'dagger'.)
Shot (8). A, B, C, E, F 2, F 3, G, W. (Once changed to 'bullet'.)
Cricket (5). D, E, F 2, F 6, V.
Dinner (5). A, B, F 3, G, W. (Once changed to 'lunch' and once to 'tea'.)
Mass (4). H, J, V, W.
Tired (4). C, D, F 3, G. (Once changed to 'sitting in a chair', and once to 'resting'.)
Stairs (3). C, F 3, W.
Apple pie (2). F 2, W.

Table II gives the details for all of these survivals.

Table II

Group	Description	No. of subjects	Points surviving
A	Adult males	14	Butler, dinner, shot, dagger
B	Priests	10	Butler, dinner, shot
C	Males at Notting Hill	10	Downstairs, tired, shot, dagger
D	Males at St Andrews	15	Tired, bowling, dagger
E	Males at Westminster	29	M.C.C., shot, dagger
F 1	Soldiers	8	Church
F 2	Soldiers	8	Cricket, tea, shot
F 3	Soldiers	6	Resting, cricket, dinner, apple pie, bullet, downstairs
F 6	Soldiers	7	Cricket, dagger
G	Boys at Nuneaton	13	Sitting in chair, shot, knife, dead
H	Boys at Clapham	10	Mass, knife
J	Scouts at Notting Hill	10	Mass, killed
K	Scouts at St Andrews	10	Fell, knife
V	Adult females	15	Mass, cricket
W	Females at St Andrews	10	7.30, Mass, downstairs, lunch, apple pie, shot, dagger

Now if we take these survivals as indicating points which are of sufficient general interest throughout a group of similarly constituted persons to resist all risk of individual deletion, one or two matters emerge which would be worth following up in other experiments. Clearly the murder incident has no restricted or specialized appeal. It is outstanding for all groups. It disappears completely in one case only, and that, curiously enough, is one of the soldier series. In three other cases it remains in a more or less disguised form. Reference to cricket remains in the two student groups, in the two soldier groups and in the adult women group. Dinner remains fairly prominently in the group of educated adult men, and in the group of priests. It stays also in one of the longer soldier groups, and is present in the final versions of both smaller groups. The meal reference persists for the group of women students, but has changed to a lunch. Mass stays in with the juniors, one group of which, as already mentioned, were closely connected with Church activities, and with both women groups. All of these instances may, possibly, be connected with fairly widespread special group interests. The other survivals are less easy to connect with any influence of this kind. There are enough indications to suggest that the matter should be pursued in further experiments.

IV. ORAL REPRODUCTIONS

An attempt was made to obtain series of oral reproductions. The story was read to one or two subjects who retold it to two or more subjects after a quarter of an hour. The process was repeated until all the subjects available in a given group had heard the story. The final subjects recorded on paper all that they could remember.

In fact nothing very new came from this change of method. The most interesting matter was that the whole experiment took on a 'jollier' character. Laughter was far more frequent, and several jokes which crept in were faithfully transmitted. There was a little less shortening, and a little more invention, particularly of the graphic and dramatic kind. Again, on the whole, the women subjects faithfully reproduced more detail than the men.

Summing up the results of this investigation we may say that, as material in verbal form is transmitted from person to person within a social group, it undergoes much shortening and rationalization. The type of explanations which remain, and the finally surviving detail, may indicate fairly widespread specialized interests of a social order. Mistakes and inconsistencies at any stage are likely to get omitted or corrected. No conclusions can be drawn with regard to the likelihood of final survival from the position of particular matter in the total material originally presented.

The thanks of the writer are due to Prof. F. C. Bartlett, who suggested the investigation, to Mr J. S. Ross, Mr E. C. Axford, Capt. J. M. Oke, and Dr O. A. Oeser who obtained some of the reproductions, and finally to all the subjects who allowed themselves to be tested.

(*Manuscript received December* 11, 1934)

THE EFFECTS OF TIME INTERVAL ON RECALL[1]

By VALENTINE HALL

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I. AIM OF WORK

THE work was planned and undertaken to examine the types of changes that occur in the recall of visually perceived forms of diagrams and pictures of simple objects over a period of time, and to find out how far higher thought processes appear to be involved in the observation that makes recall possible, as well as in the process of recalling itself. It was hoped that sufficient evidence might be obtained to show whether the changes that occur in the drawings over this period of time were of a gradual or sudden nature, the sudden nature being anticipated if the variations were due to Gestalt principles. The problem has been previously studied by Wulf(7), Gibson(3), Perkins(4), Bartlett(2), and others. This research owes much to the work of such investigators, and I am indebted to Dr F. T. Perkins, of the University of Kansas, for permission to reproduce certain of his diagrams which were used in this research. It differs from those mentioned not so much in ideas as in treatment, and is an attempt to secure quantitative results as well as qualitative ones to support and confirm earlier conclusions.

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[1] This paper is based on a thesis approved for the Degree of Master of Arts in the University of London, 1935.

Furthermore, success in accuracy of recall was examined in relation to high scores in tests of general mental ability and perseveration.

II. MATERIALS AND METHOD OF EXPERIMENT

(1) *Subjects*

The main experimental work was carried out and the figures calculated from protocols obtained from 100 boys and girls whose ages ranged from 11·0 to 12·0 years. The children were drawn from five elementary schools belonging to the Manchester Education Committee, who kindly made this investigation possible. Later the tests were given to seventy 9-year-old children and forty 15-year-old subjects for the purpose of comparing results. Twelve adults, most of them post-graduate students from King's College, also took part in the investigation, but for these subjects the technique of the experiment was slightly modified.

(2) *Material*

The material employed, all of which was visual, was of two kinds, one of which was designed to be as meaningless as possible, and the other meaningful. The latter material consisted of six pictures of objects in common use in the children's homes, and was chosen in the hope that it would be associated in the minds of the children with experience of their everyday lives. The outlines of diagrams and objects were drawn in black ink on sheets of cardboard, 20 × 25 in. There were twelve cards in all. Two sheets of black cardboard were used as a screen for the cards. Reproductions of the drawings were made in pencil on sheets of 8 × 6½ in.

(3) *Procedure*

The tests of recall were given to groups of 30–50 children, but over 50 per cent. of the 220 children who began the tests did not complete them owing to absence from school, and their drawings were not used. All children had before them six slips of paper, and on each they wrote their name, serial number, and number of test. This they did before each test.

The drawings were placed on an easel in front of a blackboard, and each set of three was covered by a screen. The following instructions were then given: "You are going to be shown a group of drawings. Look at them carefully so you can draw them when you are asked to." The experimenter said 'Ready', removed the screen, and exposed each of the three drawings for 20 sec. When the first set had been shown, after an interval of 20 sec. the experimenter gave these instructions: "Now draw what you have seen in any order on separate sheets of paper." Identical procedure was adopted for the second group of three, but if the first group consisted of diagrams, this second group would consist of pictures of objects, and *vice versa*. The drawings were then collected. Two days later, the experimenter visited the schools again, and papers were given out, named and numbered. But this instruction was now given: "I want you to draw me the pictures I showed you last time." The five sets of reproductions that were obtained were made on the day of exposure, and on the 3rd, 7th, 16th and 27th days after the pictures had been shown.

When these tests were completed, the other two groups of drawings were shown and reproductions made by the children. A different order of exposition was maintained in the different schools to minimize any differences in recall that might be accounted for by the novelty of the task in the case of the first six drawings. At the close of the experiment children from schools B, C and D were interviewed individually and asked how they were able to remember the drawings so well. Adult subjects were given the tests individually, and were asked to write introspections. The drawings were only shown them for 10 sec. and a drawing of a slipper was substituted for that of a saucepan.

(4) *Scoring*

An arbitrary system of marking was used in scoring whereby marks were allocated so that it was theoretically possible for any individual to obtain 500 marks for his 60 reproductions, 250 marks for diagrams, and 250 marks for pictures of objects. Each individual drawing was marked according to its fidelity in reproducing the original. Marks were then subtracted from the maximum scores for each change observed, such changes falling under the categories of orientation, omission, addition, and alteration.

(5) *Inclusion of* 'g' *and* 'p' *tests*

All the children were tested for '*g*' and '*p*'. The test used for '*g*' was Spearman's 'Measure of Intelligence' (6); the tests used for measuring perseveration were those described by Pinard (5). Only three out of the four tests were given, the inverted mirror test being considered less satisfactory for young and unpractised subjects. There was no preliminary practice.

III. RESULTS

(1) *Typical changes*

Many and varied as the changes were that appeared in the reproductions, they could be classed under two heads. Examples of successive changes are shown in Fig. 1.

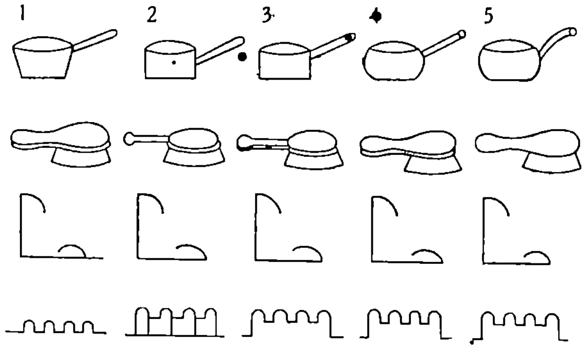
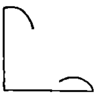
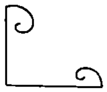


Fig. 1. Examples of successive changes of diagrams and pictures. Figures represent number of the reproduction.

Most reproductions of diagrams showed a tendency towards a symmetrical treatment of the original, as found by Perkins (4). It seemed as if the subjects were striving to effect a unity of form or structure in their drawing, which satisfied them only when it was achieved. The frequent occurrence of stabilized changes in the later reproductions strengthened this assumption. Changes were generally carried out by processes of 'sharpening' and/or 'levelling', as observed by Wulf (7). In some cases both 'sharpening' and 'levelling' were at work: one feature in a diagram was exaggerated, and another feature weakened or omitted, *e.g.*

 became 

Changes in the reproductions of pictures of objects often resulted in portraying objects with different characteristics from those owned by the original. Pictures of saucepans with curved sides, and jugs with straight sides were common. When the time for reproduction occurred the child drew a saucepan, brush, or hat, and incorporated into his drawing perhaps the details he had observed in the original, but frequently other details belonging to those objects as known in everyday experience. The line of the turned-up brim in the original picture of the bowler hat was remembered but reinterpreted and replaced in varying positions and in varying shapes. The line was drawn joining crown to brim, depicting a dent, and becoming a pointed decoration. It would seem that the effect of this experience of the familiar object is to force itself into the original percept and fuse with it so that the resulting reproduction is a conglomeration of past experiences.

(2) *Gradual or sudden changes*

Drawings were inspected to see if the changes occurring appeared during the sequence of reproductions, or if at an early period a sudden alteration occurred with no subsequent modification. In point of fact many series of reproductions did not fall definitely under either heading, so the drawings were placed in two different categories only if the sudden or gradual change was seen without question. Table I shows that changes in diagram reproductions were more frequently sudden and changes in pictures of objects more frequently gradual in character.

Table I. *Different types of changes in various age groups*

Drawings	Gradual change %			Sudden change %		
	9 yr.	11 yr.	15 yr.	9 yr.	11 yr.	15 yr.
Diagrams	10·4	4·8	3·8	15·5	7·3	15·4
Pictures of objects	20·9	8·5	14·6	10·2	5·2	7·9

(3) *Analysis of errors*

A total of 12,582 reproductions out of a possible maximum of 12,600 was obtained. Many of these reproductions showed derivations from the original, and Table II indicates how the errors were distributed. For every age there were fewer marks lost for diagrams than for pictures, and the older the age group the fewer errors as a whole. Most errors were in the nature of alterations in all groups. The number of errors allotted to the columns of omission and addition varies according to the nature of the material. With diagrams details tend to be omitted rather than added, whereas with the pictures of objects, addition of details is more frequent than their omission.

Table II. *Marks subtracted for errors*

Age	No. of subjects	Omission	Orientation	Addition	Alteration
		Diagrams			
9	70	842	676	529	1287
11	100	650	865	436	1631
15	40	223	290	59	371
		Pictures of objects			
9	70	753	748	941	1708
11	100	715	575	1097	2083
15	40	137	81	248	667

(4) *Relations with* ‘p’ *and* ‘g’

Table III shows the results obtained when the scores of the tests for ‘*g*’ were correlated with the corresponding scores for accuracy for all drawings, and also for diagrams and pictures of objects separately. It is seen that the factor of general mental ability becomes of less importance in the accuracy of recall as the age of the children increases. Accuracy of recall depends on the ability to grasp certain relations between the whole and parts in the original perception, and unless this is done the chances are exceedingly remote that a complex meaningful, or a meaningless, drawing will be remembered correctly. Therefore we might expect to find the value of the correlation ($r = 0{\cdot}48$) reasonably high for the 9-year age group, since the material would present more difficulties for them than for the 11-year age group for whom it was designed. The correlation figures for accuracy in pictures of objects follow the general trend for all drawings taken together, except for the 15-year group, whose interest in the pictures was probably slight. The material is thus regarded more as a memory test, and the subjects rely on the use of concepts for reproduction to a far greater degree than before. It is possible that the older children grouped the pictures into concepts as they observed them, while the young children tended to apprehend and educe relations in each individual picture.

Table III. *Correlation of accuracy of reproductions with general mental ability*

	All drawings		Pictures of objects		Diagrams	
Age	r	P.E.	r	P.E.	r	P.E.
9	0·48	0·065	0·52	0·062	0·25	0·079
11	0·25	0·066	0·32	0·063	0·07	0·070
15	0·23	0·106	0·03	0·112	0·32	0·100

Different types of material offer different problems. Diagrams do not themselves fall under the heading of a definitely restricted class concept, unless some attempt at conceptualization is made by the subject. This attempt may be reflected in the 9-year age group by the value of $r = +0{\cdot}25$. With children of 11, both difficulties and interest in the diagrams are less, whereas for the 15-year subjects diagrams may well take on fresh meaning connected with school work.

The correlation figures for perseveration and accuracy were in no case significant. It would appear that perseveration plays no part in the accuracy of recall in the drawings of any age group.

IV. DISCUSSION OF RESULTS

Our results support a dynamic theory of remembering such as is given by Prof. F. C. Bartlett in *Remembering*. From an examination of curves of forgetting we might anticipate a consistent fall in the accuracy of recall according to the distance in time from the original exposure. On the contrary we find that in 33 cases out of the 108 possible average scores for each diagram and picture (neglecting the immediate reproduction) there is an increase in accuracy, *i.e.* 30·5 per cent. fail to conform to the 'law'. This reminiscence was first described by Dr Ballard (1). Additional evidence is provided in the large number of errors due to the addition of details which indicate that the recall although originating in the early stimulus is a fresh reconstruction of that past experience. Two factors emphasizing this view of remembering showed themselves continually during inspection of our results, and evidence for the influence of personal attitude and higher thought processes is strong.

(1) *Personal attitude*

The practice of verbalizing the drawings during the original perception, and the making use of such verbalization in subsequent recalls, reflects the importance of personal attitude. Diagrams were given more or less appropriate but highly variable names, and it was illuminating to note how these names were linked up with the subjects' interests and experience. One diagram was referred to by the 11-year-old children as 'tents', 'mountains and little mountains', 'wigwams', 'Indian tents', 'blades of grass', 'pyramids', and by the older subjects as 'teeth on a saw', 'the Alps', 'section of a range of mountains', 'cones', 'diagram of a rectified carrier wave'; another was spoken of as 'a hat on a stick', 'lamp-post', 'jockey's cap' by the younger children, and as 'top of umbrella', 'scout's hat with tail', 'hat and neck shade (kepi)' by the older group.

Adults also reported 'naming' of diagrams, and their introspections show how their recall of the various conceptualized drawings was accompanied by personal reminiscence:

"The second exposition again was taken in as a series of geometrical figures, and the triangles related to one another. Then their similarity to 'tents' came, and the figures were fixed in mind as 'tents'."

"Then drew the series of little mountains and the arches."

Moreover, personal judgments and criticism of their own reproduction appear in their reports:

"First the tendency to draw a Grecian lamp. Knew at once to be wrong, so corrected it and reproduced as given."

"I believe these lines were more complicated than those I have drawn."

(2) *Higher thought processes*

Many reproductions given were mutilated and 'telescoped' versions of the original. The question immediately arises as to whether the alteration is due to interference by another sensory pattern alone, telescoped with the original stimulus, or if it is to be accounted for chiefly by the use of conceptual thought. Our results support the latter interpretation. Reproductions show that the influence of the everyday object is paramount, for we are faced with every species of jug, hat or candlestick. Fusion of images if present may surely be accompanied by higher thought processes in which the subject relies on conceptual knowledge to construct his reproduction. Memory images and conceptual thought may well be inextricably woven. Reports from adults emphasize this reliance on previous experience, details being added or omitted according to the subject's general concept of the stimulus:

"I'm afraid some details were put in through knowledge of the object rather than through faithful distinct recall of the actual perception."

"Found it difficult to visualize the hat. Brim seemed more ridiculous than ever when I tried to draw what I remembered, and did the brim project in front—memory said it didn't—but this was impossible. Must have been a small brim in front."

Naming of diagrams (p. 47) presupposes the employment of an analogy for the production of the satisfactory correlate, and this in turn implies conceptual thought. To see a semi-meaningless diagram as a jockey's cap entails not only the perception of the diagram but the eduction of a relation of likeness between this diagram and a jockey's cap and subsequently the using of this analogy in recalling the diagram.

Quantitative results provide another source of evidence. The fact that general mental ability influences the reproductions of many drawings, especially where the recall involves difficulties including analysis of a complex form, has been discussed (p. 46). Again, the marks subtracted for adding details (p. 45) in the reproductions of figures of objects favours the contamination of the memory image by past experience and emphasizes the use of conceptual thought.

Lastly may be cited the manner in which changes occur (p. 45) which may indicate the influence of thought processes. When there is little contact between the original stimulus and the subject's everyday experience, as in the case of semi-meaningless figures, the changes in reproductions tend to be sudden. Here conceptual thought having verbalized the original as 'hat' or 'envelope' has little more to do. Changes in reproductions of pictures, however, are more gradual, often increasing with each successive reproduction (p. 44). This type of material is only too readily associated with everyday experience, and higher thought processes are continually employed in comparing, contrasting, and educing necessary relations. It would appear that higher thought processes are frequently involved in the original observation and in the recall itself, and the important part played by interest in remembering only serves to strengthen our evidence.

V. SUMMARY

1. Changes in the reproductions of pictures of objects showed the strong influence of conceptual knowledge upon recall. Changes in the reproductions of diagrams showed a tendency to produce a symmetrical figure.

2. Changes in the reproductions of pictures of objects were more often gradual, and changes in the reproductions of diagrams were more often sudden in character.

3. Evidence indicated that higher thought processes were intimately involved in the original observation that makes recall possible, as well as in the process of recalling itself.

4. A positive relation between degree of intelligence and accuracy of recall, especially in cases where the material was of interest to the

children, was evident. No relation between perseveration and accuracy of recall was discovered.

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TEMPERAMENTAL DIFFERENCE IN TWINS

By MADELINE KERR

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  (*c*) *General* (p. 58).
References (p. 59).

I. INTRODUCTION

(*a*) *Differences of intelligence in twins.* Herrman and Hogben (1) found that when intelligence quotients of twins are compared by the method of correlation, the coefficients are of different magnitude for the different types of twins.

(*b*) *Object of the investigation.* The object of this investigation was to assess the resemblance of different types of twins with respect to temperamental differences such as those measured by the Rorschach test.

Three possibilities may be recognized. One is, that as identical twins are as alike as it is possible for two people to be genetically, they might show the same relatively high correlation in temperamental as well as in intelligence tests. They have the same gene constitution and the same uterine environment. Fraternal twins might be more similar than sibs as they have the same birth rank, but with respect to gene constitution they are no more alike than ordinary brothers and sisters. Like sex fraternal twins might show a higher correlation than unlike sex pairs owing to the similarity of their external environment, *e.g.* manner of bringing up, schools, etc.

A second possibility is that the close genetic similarity of identical twins tends to expose them to the same situations, so that the effect of environmental factors, which would cause differences of temperament, is reinforced.

Alternatively, twins of either type have so much in common that they might tend to become unalike temperamentally as a conscious or unconscious protest.

II. DESCRIPTION OF THE TEST MATERIAL

The Rorschach test was used for the differentiation of temperament. As this test has been fully described several times recently (2, 3), further detailed description would be unjustified. For convenience the symbols used for the various types of scoring will be repeated.

W = answers where the blot is seen as a whole.
D = answers where the blot is seen in detail.
M = answers involving kinaesthetic feelings.
C = answers where the colour only is described.
CF = answers where the colour is the first and the form the second consideration.
FC = answers where the form is the first and the colour the second consideration.
F (C) = answers involving appreciation of chiaroscuro.
eDF = answers where the white spaces are considered instead of the blot.
Anat. = anatomical answers.
A % = the number of times the blot is said to resemble an animal expressed as a percentage.
H % = the number of times the blot is said to resemble a person, expressed as a percentage.
Ob. % = the number of times the blot is said to resemble an object, expressed as a percentage.
Ban. % = the percentage of banal answers. Answers which occur in the ratio of 1 in 6 are counted as banal.
Orig. % = the percentage of original answers. Answers are counted as original if they occur in the ratio of 1 in 100.

III. DESCRIPTION OF THE SUBJECTS

The twins tested with the Rorschach test were all subjects who had been used in the previous investigation by Herrman and Hogben (1). The diagnosis as to which type of twin they belonged had been made by means of the finger-print method. Their intelligence quotients had been found and some physical measurements taken.

Other temperamental tests and more physical measurements were recorded during this investigation. They will be reported elsewhere.

The ages of the twins ranged from ten to seventeen years. They all came from a similar social environment in so far as they all at one time attended elementary schools.

IV. EXPERIMENTAL RESULTS

(a) *Reliability of the test*

In order to find out if what the test does measure is constant, a retest was undertaken. Fifty elementary school children who had been tested a year ago (3) were retested. Table I shows the correlations between the scores on the first and second tests.

Table I

	WF	FC	C	FM	A %	H %	Ob. %	Ban. %	Orig. %
r	0·74	0·36	0·001	0·52	0·52	0·35	0·39	0·61	0·31
σ	±0·06	±0·13	±0·13	±0·10	±0·1	±0·12	±0·12	±0·08	±0·13

The results of this table may be summarized as follows:

(1) The number of whole answers (WF) are naturally related because they correlate with I.Q. according to Rorschach, Beck (4), and the writer (3).

(2) The banal percentage (showing reliance on public opinion), the kinaesthetic (FM showing creative ability), and the animal percentage (a measure of mental stereotypy) show relatively high correlation. The inferences from these figures are that suggestibility to public opinion, creative ability, and stereotypy of mind remain moderately constant. In the opinion of the writer it would be better to say that the FM responses measure creative capacity rather than creative ability; while the capacity for creative work might remain constant, the ability would obviously fluctuate with the physiological and emotional state of the individual.

(3) Original percentage, human percentage, object percentage, and form colour responses show only low correlation. According to Rorschach

percentage of original responses would correlate with intelligence. The other criteria FC, H %, Ob. %, represent attitudes, *e.g.* FC answers are supposed to show emotional stability, H % interest in people, Ob. % objective behaviour.

(4) The sum of the three types of colour answers shows no correlation at all. If the colour responses do demonstrate fluidity of affect, there is no reason why they should be constant, since intensity and reactibility of affective response would vary with the emotional attitude of the subject at the period of the test.

(*b*) *Correlations with age*

The most important criteria were correlated with age. Two coefficients were calculated for each classification. The data for one group, the A group, were taken from the first of each pair of twins, the data for the B group from the second of each pair. Table II shows the coefficients and standard deviations.

Table II

	W		FM		A %		H %	
	A	B	A	B	A	B	A	B
r	0·098	0·26	0·071	0·154	−0·32	−0·76	0·032	0·17
σ	±0·09	±0·08	±0·09	±0·92	±0·084	±0·039	±0·094	±0·09

	Ob. %		Ban. %		Orig. %	
	A	B	A	B	A	B
r	0·113	0·016	−0·101	0·188	0·158	0·148
σ	±0·094	±0·094	±0·93	±0·091	±0·091	±0·092

The magnitude of the score for any of these classifications is evidently independent of age with the exception of the animal percentage scores. The animal percentage is supposed to vary inversely with intelligence.

(*c*) *Twin resemblances*

In the Rorschach test, unlike intelligence tests, there is no one score at the end. Therefore the various classifications of response have each to be dealt with separately.

(1) *Psychological types.* Rorschach claimed that the test divided people into four main types, introversive, extratensive, koartive, and ambiequal. So few ambiequals were found that they have not been taken into account in this investigation. Table III shows the number of pairs of like or unlike psychological type for each variety of twin, *e.g.* if both members of a pair were koartive then they are of like psychological

type; if one were koartive and the other introversive then they are of unlike psychological type.

Though the identical twins show a larger percentage of pairs belonging to the same psychological type than the other twins or unrelated pairs, the differences between the percentages are not large enough to be statistically significant.

Table III

	KK %	KI %	KE %	EE %	EI %	II %	Like pairs %	Unlike pairs %
Identical twins (27)	33·3 ± 0·9	25·9 ± 8·3	7·4 ± 5·0	0	14·8 ± 6·9	18·5 ± 7·7	51·8 ± 9·6	48·1 ± 9·6
Fraternal twins like sex (38)	21·07 ± 6·1	34·2 ± 7·1	5·2 ± 3·2	0	15·7 ± 5·8	23·6 ± 6·9	44·67 ± 8·06	55·1 ± 8·03
Mixed fraternal twins (49)	30·6 ± 6·5	48·2 ± 7·1	2·04 ± 2·0	2·04 ± 2·0	6·1 ± 3·4	16·3 ± 3·6	48·9 ± 7·1	51·69 ± 6·6
All twins (114)	28·07 ± 13·3	35·9 ± 4·4	4·39 ± 1·5	0·8 ± 0·84	11·4 ± 2·9	19·3 ± 3·4		
Unrelated pairs (114)	19·2 ± 3·6	43·8 ± 4·6	10·5 ± 2·9	0	7·8 ± 2·5	18·4 ± 3·6	37·6 ± 4·8	62·1 ± 4·8
Random expectation	22·8	41·1	8·8	0·84	7·8	18·9		

(2) *Sex difference.* An attempt was made to see if there was any sex difference in the distribution of psychological types. Table IV shows the result.

Table IV

	KK %	KI %	KE %	EE %	EI %	II %	Like pairs %	Unlike pairs %
♂♂ (21)	9·5 ± 6·3	57·1 ±10·7	4·7 % ± 4·5	0	4·7 ± 4·5	23·8 ± 9·2	33·3 ±10·3	66·6 ±10·3
♀♀ (34)	20·5 ± 6·8	47·05 ± 8·5	8·8 ± 4·8	0	11·7 ± 5·5	11·7 ± 5·5	32·3 ± 8·01	67·6 ± 8·01
♀♂ (59)	23·7 ± 5·5	35·5 ± 6·2	13·5 ± 4·4	0	6·7 ± 3·2	20·3 ± 5·2	44·06 ± 6·4	55·9 ± 6·4
Random expectation	22·8	41·1	8·0	0·84	6·4	18·4		

From this table it appears that like sex twins are not necessarily of the same psychological types, as there are more unlike than like pairs in all categories.

(3) *Distribution of whole responses.* A classification into four groups was made in the case of the whole responses:

Group A consisted of over 10 whole responses
Group B „ 7 to 9 „
Group C „ 4 to 6 „
Group D „ 0 to 3 „

The following percentages of the number of pairs of twins falling into like and unlike groups were found:

	Like pairs	Unlike pairs
Identical twins (27 pairs)	44·5 ±10·8	55·7 ±10·8
Fraternal, like sex (38 pairs)	41·9 ± 7·9	57·6 ± 7·9
Fraternal, unlike sex (49 pairs)	38·6 ± 6·9	61·07 ±6·9

There are more like pairs among the identical than the fraternal twins, and more like pairs among the fraternal like-sex pairs than among the fraternal unlike-sex pairs. However, these differences are not big enough to be significant.

(4) *Distribution of kinaesthetic responses.* An attempt was made to group the kinaesthetic responses in a similar manner. Rorschach's grouping was adhered to as much as possible. The result was wholly inconclusive and insignificant.

(5) *Distribution of banal percentage responses.* The banal percentage responses were grouped as follows:

Group A	is from	0	to	14 %
Group B	,,	15	,,	28 %
Group C	,,	29	,,	42 %
Group D	,,	43	,,	56 %
Group E	,,	57	,,	70 %

The following percentages of like and unlike pairs were obtained:

	Like pairs	Unlike pairs
Identical twins (27 pairs)	40·7 ±9·4	59·1 ±9·4
Fraternal, like sex (38 pairs)	31·5 ±7·5	68·4 ±9·4
Fraternal, unlike sex (49 pairs)	34·6 ±6·7	65·3 ±6·7

Here again the differences are not large enough to be statistically significant.

(6) *Distribution of identical replies.* It was thought possible that identical twins might give a higher percentage of identical replies than fraternal twins. The number of identical answers given to the same blot by each pair of twins was counted. The following results were obtained:

	Means of identical responses
Identical twins	3·37 ±2·3
Fraternal, like sex	3·39 ±1·8
Fraternal, unlike sex	3·32 ±1·8

The differences are obviously insignificant, so apparently like genetic constitution and similar environment do not cause identical replies to the blots.

(*d*) *Correlations with other tests*

Criteria which theoretically might have some relation to other tests were correlated with the scores obtained by the twins on the Rorschach test.

(1) *Correlation with the Otis Group Intelligence Test.* The number of whole, kinaesthetic and original percentage responses, which are supposed to indicate intelligence, were correlated with the I.Q. obtained on the Otis test:

	WF	FM	Orig. %
r I.Q.	−0·0115	−0·001	−0·00005
σ	±0·066	±0·9	±0·99

The criteria which Rorschach says measure intelligence have therefore no relation with the ability measured by the Otis test. The writer, in a previous paper (3), found a correlation of 0·57 ± 0·045 between the number of whole responses and I.Q. on the Otis test on 100 children. No explanation can be offered for this discrepancy.

(2) *Correlations with tests of perseveration.* The animal percentage responses, indicating stereotypy of mind, and the banal percentage responses, indicating dependence on public opinion, were correlated with the average scores obtained from a battery of tests designed to measure that type of perseveration designated as 'p'.

The subjects were given SS to write for 30 sec., more SS for the next 30 sec., ƧƧ for the next 30 sec. and SƧ for the next 30 sec.[1] They were allowed to practice four of each figure before each period of 30 sec. The same technique was used for Δ, e, Z, 9, 2, 6, G.

The scores were obtained by dividing the sum of the first two periods by the sum of the second two, *e.g.*

$$\frac{\Sigma(S_1+S_2)}{\Sigma(Ƨ+SƧ)}.$$

The average of the scores of the eight tests were used for the purpose of correlation. The following results were obtained:

	A %	Ban. %
$r\,P$	0·00001	−0·0001
σ	±0·99	±0·99

Neither stereotypy of mind nor dependence on public opinion as measured by the Rorschach test has any statistical relation with perseveration as measured by these tests.

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[1] These results were obtained by Dr E. Pratt Yule (1935), *J. Mental Science*, LXXXI, 334.

(e) *Inter-twin correlations*

As the correlations with age were insignificant some inter-twin correlations were calculated. The results were insignificant, *e.g.* for the number of whole responses the coefficient was 0·016 ± 0·09, for banal percentage responses 0·16 ± 0·092.

V. CONCLUSIONS

The object of this investigation was twofold:

(1) To find out if twins resembled each other in temperament as they do in intelligence.

(2) To explore the reliability of the test.

(a) *Twin differences*

(1) More identical twins belonged to the same psychological type than fraternal of like or unlike sex. The difference was not very great, except between identical twins and unrelated pairs.

(2) Sex does not apparently influence psychological type.

(3) Correlations of one member of a pair of twins with the other were found to be insignificant.

(b) *Reliability of the test*

The test appears to be reliable for the number of whole and the banal percentage responses, and fairly reliable for kinaesthetic and animal percentage responses.

In view of previous work it is difficult to dismiss the test as unreliable. It is possible that, with the exception of banal percentage responses, most of the criteria of assessment measure mood or attitude and not temperament.

(c) *General*

As the test measures differences of temperament or mood independent of those measured by scores on intelligence tests, then,

(1) the comparatively low order of similarity of identical twins, and

(2) the small difference in the order of similarity for different types of twins,

suggests that the differences measured by this test are not exclusively due to differences in genetic equipment but arise from the interaction of temperament of individuals of similar genetic constitution. This seems to provide a roughly qualitative basis for the concept known as protest.

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THE TIME PATTERN OF VOLUNTARY MOVEMENTS

BY W. PETERS AND A. A. WENBORNE

(*Continued from* Vol. XXVI, pp. 388–406)

IV. *Polyphasic movements* (pp. 60–66).
V. *Amorphous movements* (pp. 66–71).
VI. *Summary* (pp. 71–72).
References (p. 73).

IV. POLYPHASIC MOVEMENTS

AT an early stage of our experiments we wished to study the time pattern of long voluntary movements. Up to then the longest movement had been an arm movement over a horizontal track of 30 in. As we wished to avoid making simple movements to and fro or repeating the same movement continuously, we chose a movement over a spiral track.

We constructed such a track with small strips of zinc, making it three times as long as the straight track (Fig. 2). The subject was instructed to move the stylus at maximum speed from a point in his median plane marked 'start' over the spiral track of zinc strips and to lift it off at the end of the track. The movement was made with the right arm. Although the arrangement allowed for fifty-six sections to be measured, only ten zinc strips were connected up with the chronoscope. These sections are marked I–X (Fig. 2).

Every section was measured ten times with Subjects We and Pe, so that every subject carried out a hundred tests. The zinc strips were 1 in. wide, half that of the strips employed in the earlier tests, and 1 or 2 in. long. Only the length of the strips is considered in measuring the track. Within a certain limit the width of the strips is immaterial. We used strips 1 in. long in order to obtain a curved track. There were twenty-eight 2-in. strips and thirty 1-in. strips. Four of the 1-in. strips were connected in pairs; thus there were fifty-six sections in all. They could not be spaced equally on account of the curvature of the track, but the gaps were so small as not to impede the movement.

We selected the ten sections over which the velocity was to be measured with two ends in view. First we chose sections near the starting-point, in the middle and near the end of the track. Then we chose those sections over which the movement took place in various directions, *i.e.* away from the body, forwards or outwards and back towards the body. If we imagine the zinc strips laid in a straight line without any space between them, the distance from the starting-point to the beginning of section I was 2 in., to section II 10 in., to section III 21 in., to section IV 36 in., to section V 39 in., to section VI 45 in., to section VII 63 in., to section VIII 73 in., to section IX 78 in., and to section X 82 in. The total distance from the starting-point to the end of the track was 86 in.

The zinc strips were fixed to the table in such a way that the starting-point was in the median plane of the subject, about 8 in. away from his body. As the movement covered the whole of the track, the subject only had to be reminded that the movement was to be carried out at a maximum speed before the signal for the start was given. He did not know what part of the track was being measured. The experimenter connected up alternately one or the other of the ten sections.

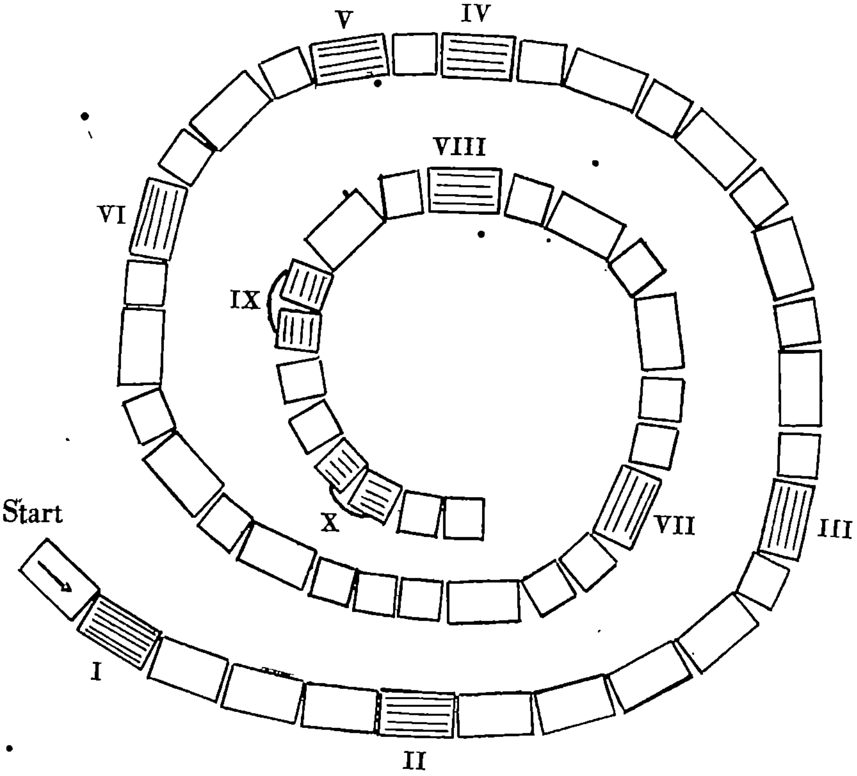

Fig. 2.

Both subjects had the impression that the maximum speed of the movement over the spiral track was considerably less than that over the straight track. Subject We stated, moreover, that she had to be careful not to let the stylus slip off the zinc track. Subject Pe occasionally slipped off and had to repeat the movement from the starting-point. The subjects also observed that the movement went very smoothly sometimes and haltingly at other times.

The times taken over the shorter sections of the spiral track, measured objectively, were considerably longer than those taken over the straight track. The ten sections of the spiral track over which the times were measured had a total length of 20 in. Adding up the average times taken over these ten sections, Subject We took 67 $\frac{1}{100}$th sec. and Subject Pe 63 $\frac{1}{100}$th sec. This was three times as much as the time taken by Subjects We (2) and Pe (2) over 20 in. on a straight track. As we did not measure the times for starting and stopping on the spiral track, we had to compare the times taken over 20 in. on a straight track also without the starting and stopping times. This was done by adding the times taken over sections II–V over a straight track of 30 in. This worked out at 25 $\frac{1}{100}$th sec. for We (2) and 18 $\frac{1}{100}$th sec. for Pe^2. Even at ordinary speed the times over a straight track were only about half of those taken at maximum speed over a spiral track. The considerably longer times over the spiral track were, however, not so much due to a uniform slowing down of the whole movement, as was the case on the straight track at ordinary speed, as to a slowing down in certain sections of the spiral track.

If the movement over the spiral track were a 'motor impulse effect' operating over a longer track than that of the previous experiment, we should have expected that the times taken over the first half of the track would decrease progressively and increase again towards the end of the track. No such change occurred, however, as shown in Table VII which gives the average times in $\frac{1}{100}$th sec. taken over sections I–X by Subjects We and Pe. With both subjects the velocity increased and decreased alternately in the course of the movement, but we did not find a regular progressive increase over the first half nor a similar decrease in the second half of the track.

Table VII

Section		I	II	III	IV	V	VI	VII	VIII	IX	X
Distance from the starting-point in ins.		2	10	21	36	39	45	63	73	78	82
Average time in $\frac{1}{100}$th sec.	We	6	5	11	5	4	8	7	6	11	4
	Pe	5	4	7	7	6	7	7	5	9	6

It should be noticed, however, that the times taken by the two subjects vary in a certain way over certain sections. Over sections III, VI and IX the velocity diminished, while it increased over the sections lying between sections III, VI and IX, and it also increased before section III and after section IX. Looking at Fig. 2, we see that at sections III, VI and IX there were noticeable changes in the direction of the movement, and it was at these points that the velocity decreased. But this was not the case at every point where the direction of the movement changed. In section VII where the movement changed from an outwards to a forwards direction our measurements did not clearly reveal a decrease in velocity, nor did they do so where the movement changed from a forwards to an outwards direction. This may, however, be due to the fact that we measured the times only over a small number of selected sections.

We pointed out above that earlier studies of voluntary movements revealed changes of velocity on changing the direction, especially in movements of long duration. They showed a decrease at a change of direction or the formation of new rises and falls in the velocity curve. Loeb and Korányi(1) who found this to be true in movements lasting more than 2 sec. stated that it looked as if new impulses had been received. Wachholder(2) found in most cases of slow movements alternate increases and decreases in velocity. He said that some movements appeared as if built up by stages from several smaller movements. We, too, were driven to the conclusion that the movement over the spiral track may be composed of several movements of the same kind as those previously described.

If a movement like the 'motor impulse effect' which shows a velocity curve rising from a minimum to a maximum and decreasing again can be described as a movement of one phase, a movement like that over the spiral track, the velocity curve of which shows several minima and maxima, can be described as a movement of several phases (polyphasic). Although the whole movement over the spiral track cannot be looked upon as a 'motor impulse effect', it is possible that its various phases are such 'motor impulse effects' showing characteristic velocity curves between the minima caused by starting and stopping. The movement over the spiral track would thus appear as a chain of 'motor impulse effects' linked together in order to reach the goal of the movement which could not be accomplished by one 'motor impulse effect'. In order to prove this hypothesis correct we carried out some more experiments as described below.

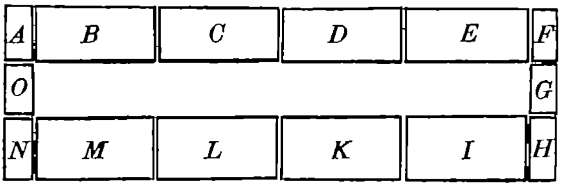

Fig. 3.

We constructed a rectangular track as shown in Fig. 3. Its length from the left-hand edge of section A to the right-hand edge of section F was 22 in., not counting the small spaces between the zinc strips; and its total length throughout was 48 in. The distance from the centre of section A to the centre of section N was 4 in. Strips B, C, D, E, I, K, L and M were each 5 in. long by 2 in. wide, strips A, F, H and N were 1 in. long by 2 in. wide, and strips G and O 2 in. long by 1 in. wide.

The subject was instructed to make a hand-arm movement along three sides of the rectangle at maximum speed. The movement was made alternately from section A over F, G and H to the left edge of section N, and from F over A, O and N to the right edge of section H. In the first case the movement was clockwise and in the second case anti-clockwise. Either movement consisted of a long movement away from the body, a similar one towards the body and a short movement at right angles to the former, and thus had to change its direction twice. Each of the sections A–F and H–N was connected up with the accumulator and the chronoscope by a separate key. The times required to cover each of the twelve sections were measured twenty times with each subject, *i.e.* ten times for the clockwise movement and ten times for the anti-clockwise movement. The times taken over the short vertical sections G and O were not measured.

These experiments were preceded by others in which we used a flat stylus as described above. It did not, however, prove very useful, as the subjects showed a tendency to turn it on changing the direction, thus interrupting the movement. In these last experiments a metal thimble on the forefinger of the right hand was used instead and was connected up with the chronoscope by a thin wire.

Before carrying out each movement the subjects were told whether to start at A (and to move *via* F and H to N) or at F (moving *via* A and N to H). Otherwise the tests were carried out in the same way as those described in section III.

Table VIII shows the results of the rectangle tests, stating the average time in $\frac{1}{100}$th sec. taken over the single sections. The table is arranged so as to allow direct comparison of the times taken over corresponding sections while moving clockwise and anti-clockwise respectively. The sections forming the corners of the rectangle are marked in heavy type. It should be borne in mind that these sections are only 1 in. in length while each of the other sections is 5 in. long.

Table VIII

		Movements away from the body over sections						Movements towards the body over sections					
		A	B	C	D	E	**F**	**H**	I	K	L	M	**N**
Clockwise movements	We	6	6	3	3	8	6	5	7	3	3	6	19
	Pe	11	6	2	1	14	21	24	29	2	2	8	31
		F	E	D	C	B	**A**	**N**	M	L	K	I	**H**
Anti-clockwise movements	Pe	15	9	3	3	6	19	21	14	2	1	6	19
	We	8	6	3	3	9	6	6	4	2	3	9	18

A comparison of the times recorded for movements towards and away from the body reveals that every movement shows the typical time pattern of the 'motor impulse effect'. *With both the subjects the clockwise as well as the anti-clockwise movements are divided into two 'motor impulse effects', one away from the body and another towards the body.* We do not know whether there is a third 'motor impulse effect' for the vertical part of the movement as we did not measure the time taken over these sections. (In this case the stopping of the one and the starting of the next effect would both take place in the corner sections.) We were not so much concerned, however, with proving whether at each corner or curve of the track a 'motor impulse effect' ended while a new one started, as with finding out whether the movement was polyphasic and composed of 'motor impulse effects'. Table VIII shows that this was indeed the case.

The eight phases of the four movements in Table VIII all show the relatively long starting and stopping times as well as the increase in velocity for the first half of the movement and the decrease in the second half. The clockwise and anti-clockwise movements thus each show two minima corresponding to starting and two to stopping, and two maxima of velocity.

Some of the velocities recorded over the different sections of the track are higher than those shown in Table IV. In spite of the higher velocities the time required for each of the two parts of the track was longer with Subject Pe and not less with Subject We than over a path of the same length in our previous experiments.

We notice that in Table VIII the times taken over the penultimate section of each half of the track were disproportionately long. Although they are always shorter than the stopping times (per inch), the difference in velocity between this section and the one before is very marked. The abrupt decrease in velocity over the penultimate section of each half of the track is easily explained. In our previous tests over a straight track the subject had four more inches to cover on leaving the penultimate section, whereas in this test the last section was only 1 in. in length.

Moreover, in the previous tests the movement often went beyond the stopping-point. In the tests over the rectangular track this had to be avoided and the finger rarely went off the track. When this happened the movement stopped on the wooden base and the test was invalid. The subjects thus had to be careful not to let the finger slip off the track, which could only be avoided by decreasing the speed of the movement over the penultimate section, *i.e.* before reaching the corner section. Such a decrease in velocity could only be brought about by an anticipatory adjustment. It cannot have been made at the moment when the subject perceived that he was nearing the goal, as the movement would have passed that point by the time he had become aware of the fact. The movement must have been regulated differently in this case, where it was necessary to stop in good time, from the occasions described above, where it did not matter so much if the movement went beyond the fixed goal.

Subject Pe took a long enough time at the end of the first and the beginning of the second phase to have lifted the finger from the track and to start the movement again, as at the start and the end of the movement. The finger was, however, never lifted off the track at that point. Subject Pe frequently observed that the movement came to a halt for a moment at the end of the first half of the track without a special impulse to stop, but then a new impulse was required to continue the movement in a vertical direction. This impulse to continue presented itself differently to the conscious mind from the impulse to start. At the end of the vertical section the stopping and the impulse to continue were repeated. The movement thus showed three distinct phases. As the end of one phase and the beginning of another occur on one and the same zinc strip the times measured include the starting and the stopping times. They are, therefore, considerable, although part of the stopping and starting movements, *i.e.* the lifting and putting down of the finger, was omitted.

Subject Pe, as observed by the experimenter, followed the rectangular track fairly accurately. The movement turned off sharply at right angles at the corners. Subject We, however, rounded off the movement noticeably at the corners. She moved her finger in a downward curve from the penultimate section of the first half of the track, so as not to reach the edge of the last section at all. There was thus no noticeable stopping of the movement and yet there was an obvious decrease in velocity over the last section. The stopping and the starting of the movement over the last section did not take nearly as long as with Subject Pe, but it took nearly as long as the first start of the movement. The subject sometimes became aware of an impulse to continue the movement, but not in the majority of the tests. The two-phase character of the movement was thus shown more clearly by objective measurements of the times taken over the various sections than by subjective observations.

V. AMORPHOUS MOVEMENTS

The last group of tests described in this paper concerned movements carried out over the track shown in Fig. 1 (see this *Journal*, XXVI, 389) with the stylus used in the earlier tests. These movements are distinguished from those carried out hitherto in that their path was absolutely prescribed beforehand. We therefore refer to them as 'precision movements'. In all the previous tests there were various paths on the not-too-narrow track which the movement could take to reach the goal. Given the instruction to move in an absolutely straight line from one point to the other there is only one path which the movement could take.

As in the tests described in Section III, the time taken over the whole track as well as over the single sections was measured and the mean value calculated from 10 measurements with each subject.

The subject had to perform four different movements: (1) He had to move with maximum precision in a straight line between two points marked on the track. The movement had to be started as exactly as possible on the marked starting point and to stop on the exact point. The starting point was close to the left edge of section I.

The points were 20, 25 and 30 in. away from each other. We thus measured the times over 5, 6 and 7 sections apart from the time taken over the whole track.

(2) He had to move with maximum precision in a straight line between three given points. The movement was to begin exactly at the starting-point in the median plane, go through the second point and end at the third given point. The distance from the first to the third point was 5, 15 or 25 in. With a path of 5 in. the second point was marked near the left-hand edge of section II, with a path of 15 in. near the left-hand edge of section III, and with a path of 25 in. near the left-hand edge of section IV.

(3) The subject had to move with maximum precision in a straight line from a given starting-point to a given finishing point, passing through several points marked on the track at intervals of 5 in. The intermediate points were marked near the left-hand edges of sections II–VII. The movement over 5 in. passed therefore through one point only, the movement over 15 in. through three, over 20 in. through four, over 25 in. through five and over 30 in. through six points.

(4) A straight line was drawn in chalk on the track parallel to its edges; on it distances of 20, 25 and 30 in. were marked. The starting-point was marked about ⅛ or ⅙ in. under the beginning of the chalk line. From there the subject was to move as precisely as possible in a line parallel to the chalk line and to stop exactly below its end.

In the first tests we tried to measure in how far the subjects succeeded in performing the task, and to determine the deviation of the actual from the intended movement. Since we found, however, that we could not obtain an exact comparison between the intended and the actual path we did not continue these measurements.

In each of the four tasks the subjects were instructed to carry out the movement with the maximum of precision as well as at maximum speed, but it was pointed out that precision should be their first aim. We already stated above that Subject Pe never succeeded in combining the two objectives and that Subject We found it very difficult. Introspection by Subject Pe revealed that frequently the intention regarding precision was lost in the course of the movement which went on automatically. As shown by objective measurement, Subject We solved the task of combined precision and speed much better than Subject Pe. Her times were, on an average of the four series of tests, not much more than a third (37 per cent.) of those of Pe, but she stated, nevertheless, that the two intentions interfered with one another.

Naturally the precision movement took much longer than the movements described so far. The retardation as compared with the 'motor impulse effect' was most marked, however, in the movement parallel to the chalk line. The total time taken by We over the paths of various lengths was three to four times as long as in series We (2), and by Pe ten to thirteen times as long as in series Pe (2). The next highest retardation was shown with the task of moving in a straight line without passing through an intermediate point. We took twice to two-and-a-half times and Pe five to seven times as long as in series We (2) and Pe (2) respectively. The least retardation is shown in the movement through one intermediate point. Pe took four to five times as long as in series Pe (2), but We showed no difference in time as compared with series We (2) over short paths, and a very slight increase over longer ones. Most of the times taken with the precision movements were so long that it was possible for sensory control to take place. Probably a certain amount of sensory control is necessary to carry out a precision movement. The subjects frequently stated that the movement became 'automatic,' thus losing in precision; these movements generally took especially short times. Introspective observations very frequently pointed to visual and kinaesthetic control.

On the whole there seemed to be more kinaesthetic than visual control. In carrying out movements in a straight line without intermediate points, the subjects had the impression that the movement, once started, was controlled kinaesthetically only. At the start visual control also was exercised, and was employed again towards the end. Kinaesthetic control also did not seem to function continuously but to occur at more or less frequent intervals. Sometimes it seemed to rob the movement of its uniformity. The considerable retardation of the precision movement as compared with other movements is probably due to the sensory control and it may also be the reason for the difficulty in combining speed with precision.

Introspective observations showed that two typical forms of movement were being employed: one *a quiet, steady movement* of the arm and hand, the other *a series of jerks*. In the latter there seemed at intervals to be periods of latency or retardation. It might have been thought that the first form would occur with movements favouring a steady progress, such as straight movement without intermediate points or movements parallel to a given line, whilst the movement through several points would favour jerkiness. This was, however, not the case. We observed both forms of movements alternately in each of the four tasks mentioned above.

The time pattern of precision movements showed a certain variety, sometimes corresponding to those described above, sometimes different from them. There was, however, no new pattern clearly shown, nor one of those previously mentioned. The only common features in the various patterns of precision movements were slight increases and decreases in the times taken over the various sections of the track, which did not, however, occur with any regularity. Although there were sometimes traces of certain time patterns, the precision movements did not show a clear and regular time pattern. We therefore called them amorphous movements.

This becomes apparent when looking at the figures given below for the times (in $\frac{1}{100}$th sec.) taken by Subjects We and Pe in moving parallel to a given line over a track of 30 in.

We ...	25	50	37	36	41	43	45
Pe ...	59	110	84	110	119	109	109

The short starting times result from the shortness of section I (1 in. as compared with 5 in. for the other sections). From section II on, both subjects show first a slight increase and then a slight decrease in velocity. The variations in velocity are, however, much slighter than was the case in the 'motor impulse effect'. This is shown clearly in comparing on a common basis the times taken by parallel movements with those of the 'motor impulse effect'. A common basis was formed by calculating the percentage of the time taken over each section in relation to the time taken over the whole track. The result is shown in Table IX.

Table IX

		Percentage of time for whole track taken over section						
Subject	Movement	I	II	III	IV	V	VI	VII
We	Parallel movement	9	18	13	13	15	16	16
	'Motor impulse effect'	19	14	9	7	6	9	36
Pe	Parallel movement	8	16	12	16	17	15½	15½
	'Motor impulse effect'	26½	13	7½	6	7½	7½	32

The velocity curve of the precision movement is much more level than was the case with the 'motor impulse effect'. In other precision movements this levelling of the velocity curve was even more pronounced. For the movement between two points 20 in. apart Subject Pe recorded the following times, which are also given in percentages:

	53	64	53	52	56
Percentage	19	23	19	19	20

In comparing these figures with the times given in percentages taken by the same subject in a 'motor impulse effect' over a distance of 20 in., we obtain the following figures:

Percentage	33	15	8	8	36

Sometimes steady velocity was maintained up to the penultimate section, a decided decrease in velocity occurring at the end of the movement. Subject We recorded the following times with a movement passing through several points over a distance of 25 in.

	13	17	13	15	16	33
Percentage	12	16	12	14	15	31

Occasionally some of the precision movements show velocity curves retaining the time pattern of the 'motor impulse effect', as in the following times recorded by Subject We with a movement going through one intermediate point over a path of 25 in.:

11 11 8 8 10 24

They show clearly an increase up to the third section and a decrease from the fifth section. If we add up all the cases showing more or less clearly the time pattern of the 'motor impulse effect' or, at least, of traces of it, we see that they form almost one-third of the precision movements. Movements between two points show traces of this time pattern more frequently than any of the other precision movements.

Roughly a quarter of our precision tests show a certain similarity with the time pattern of two-phase movements. In a few cases we noticed it quite distinctly, *e.g.* with Subject Pe in a movement going through one intermediate point over a path of 25 in. which showed the following time curve:

37 12 9 74 6 42

This is not an amorphous but a two-phase movement. The point through which the movement was to pass was in section IV. There the first phase ended and the second phase started. Subject We, carrying out the same movement, recorded the following times:

10 8 12 11 6 21

The two-phase character is not clearly marked. This was typical for this group of movements. In this instance the second phase did not start in section IV, where the point through which the movement had to pass was marked, but had already started in section III. A similar indistinct two-phase character was observed more frequently in the parallel movement, where it was not expected, than in the three-point movement, where it would seem natural, since the path appears bisected by the intermediate point.

In a few cases velocity curves similar to a three-phase movement were observed, *e.g.* with Subject Pe moving through six points over a path of 30 in. the following figures were recorded:

37 83 47 52 58 44 46

A new phase was begun in section II and a third in section V.

With precision movements it must not be expected that certain tasks are carried out with a certain type of movement. With each of the four tasks various patterns of movement occurred. Only with the two-point movement the pattern approximating to that of the 'motor impulse effect' was predominant. Apart from that there were also patterns similar to two-phase movements.

It must not be expected either that a subject would carry out a task consecutively with the same kind of amorphous movement. When repeating after a pause of several days the series of three-point movements over paths of 15 and 25 in. Subject We produced entirely different forms of amorphous movements. In the first series the movement came near to the 'motor impulse effect'. In the second series one was almost a two-phase movement and the other showed a decrease in velocity over each section. With Subject We the amorphous movements showing a similar pattern to the 'motor impulse effect' were more frequent while with Subject Pe those similar to two- and three-phase movements occurred more frequently.

The question remains whether the slowing down of the movement in tasks of precision or the sensory control are responsible for the breaking up of the time pattern. A further question is whether the amorphous movements under different test conditions might not prove to be polyphase movements with very short phases. Both these questions can only be settled after further tests have been made.

VI. SUMMARY

We studied the time pattern of voluntary hand-arm movements by measuring with a chronoscope the times taken by the hand in moving over consecutive sections of a track. As a result we observed three types of voluntary movements: The *'motor impulse effect'*, the *polyphasic movement* and the *amorphous movement.*

'Motor impulse effects' result if we move the arm and hand at maximum speed from a given point to another along a not too narrow track without having to carry out any other task. The velocity of this movement increases over more than half the distance and decreases in the second half. The movement thus shows two minimum velocities at the start and at the finish and one maximum. With movements over shorter paths maximum velocity is reached sooner and begins to decrease sooner than over long paths. The time pattern of the 'motor impulse effect' is not affected by whether the movement is carried out with the right or the left hand, away or towards the body, from the median plane or from a distant plane. Movements at ordinary medium speed show an identical time pattern.

Polyphasic movements resulted when the hand moved at maximum speed over a spiral or rectangular track. Similar movements were also observed occasionally with precision movements over a straight track. Polyphasic movements showed several maxima and twice as many minima of velocity. Over the two longer sides of a rectangular track we observed two phases each progressing from a minimum to a maximum of velocity, and falling to a minimum again. The movement over the two long sides of the rectangular track thus showed four maxima and four minima of velocity. The time pattern of each phase approximated to that of the 'motor impulse effect', from which it differed, however, in that the decrease in velocity started at an earlier point.

Amorphous movements were observed as a rule when movements over a straight track had to be carried out with maximum precision either between two given points or parallel to a given line or passing through one or several intermediate points. It is characteristic of the amorphous movement that it lacks a definite time pattern. Where traces of a pattern can be seen, the differences in velocity over the various sections are mostly negligible. Some of the amorphous movements are similar to levelled-up 'motor impulse effects', while others resemble levelled up two- and three-phase movements.

Sensory control, *i.e.* visual and kinaesthetic control, seemed to be present in various degrees in different types of voluntary movements. The 'motor impulse effect' did not reveal any sensory control once the movement had started. The amorphous movement, however, seemed to be controlled continuously or intermittently; this was only possible at a greatly reduced rate of speed.

The 'motor impulse effect' was characterized, subjectively, as a movement which was carried out entirely automatically once the impulse had been received. The course of the movement, its speed and time pattern, had evidently been determined in anticipation by the intention.

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TASTE

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I. *Previous work on taste* (pp. 74–75).
II. *The present scientific theory and its limitations* (pp. 75–77).
III. *Proposal for another theory* (pp. 78–79).
IV. *Other sense organs subserving taste* (pp. 79–81).
V. *The structure of the taste experience* (pp. 81–83).
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References (p. 85).

I. Previous work on taste

The main purpose of this essay is to propose a scientific definition of 'taste' and partly also of 'smell', in accordance with popular use, but different from that usually employed in scientific treatises. This definition was originally proposed in 1923 in University Lectures on taste, when the connection between the doctrine of taste and the questions of food and digestion was thought to be of importance.

In recent treatises the view of earlier scientists about taste are treated with forbearance. For example, Emil v. Skramlik (1) in his fundamental treatise concerning taste and smell says: "When we read about the classification of taste qualities made by scientists of former days and shake our heads, it is our duty to remember their difficulties...they had to be pioneers in places where, thanks to their labour, there are now practicable roads."

The main difference between the contemporary and the earlier scientists is that, while the moderns only recognize four taste qualities, viz. sweet, sour, salt and bitter, the earlier workers recognize a long list of qualities. Linnaeus, the Swedish botanist, recognized ten taste qualities, viz. aqueous, acid, fatty, sweet, mucous, dry, bitter, astringent, acrid (sharp) and salt. In our opinion his main error was that he distinguished too few rather than too many taste qualities. The starting-point for every discussion of taste must be the experience of daily life. Modern science has strayed far away from this point, Linnaeus was a little less removed from it, and we return to the source.

The presentation of the accepted doctrine of taste generally starts with an account of certain sense organs, the taste buds, which were detected in 1867; these are localized on the papillae, most of which are situated on the tongue, while some are situated elsewhere in the cavity of the mouth. Children have a much larger number of papillae than grown-up people, and their papillae are also more regularly distributed. Physiologists maintain that the taste buds have been proved to be the sense organs of the four qualities, sweet, sour, salt and bitter.

It is supposed, but this has not been demonstrated, that there are separate taste buds for each of the four qualities. But it is known that the sensitivity for the different qualities varies from place to place in the cavity of the mouth and on the tongue. The distribution of sensitivity is different from individual to individual, but on the whole it can be said, for instance, that the sensitivity for sweetness is greatest on the tip of the tongue, and the sensitivity for the bitter taste is greatest at the root. A great deal of scientific labour has been expended to obtain these results; but curiously enough small children, and also some grown-up people, behave as if they knew about them. When a small child tastes something sweet, which it likes, it draws up its mouth in a way that clearly shows that it is the tip of the tongue which is chiefly used. But if a child is given something bitter to eat, which it does not like, it makes a wry face in a way that shows that it is now important to *avoid* contact with the root of the tongue. From these two reactions two general mimic expressions for things we like and for things we do not like are derived.

There is a large amount of literature about the four qualities and the functioning of the taste buds (different kinds of contrast, adaption, differential and absolute threshold, equations of taste mixture corresponding to the laws of colour mixture, etc.). v. Skramlik gives an excellent and exhaustive account of others' and his own extensive researches. It is not possible to raise any considerable objections to the individual results, but it is quite another matter with the general position of the modern doctrine.

II. THE PRESENT SCIENTIFIC THEORY AND ITS LIMITATIONS

The first positive part of this doctrine is that the four qualities constitute the whole domain of taste, and that the taste buds are the sense organs. The second part is, that most of that which ordinary people call taste is not really taste at all, but belongs either to smell or is concerned with the tactile sense organs in the mouth.

The fact brought forward to corroborate this view of taste and smell is curious and interesting. It amounts to this: Much of what ordinary people call taste originates in the stimulation of sense organs situated in the cavities of the nose, due to the passage through the nose either of expiratory air which has been in contact with food in the mouth, or of inspiratory air which has been in contact with food during its approach to the mouth. It is easy to demonstrate this by pinching the nose with the fingers when tasty food is being eaten. After some seconds the taste of the food becomes less rich: the coffee ceases to taste of coffee, wine loses its aroma, and a melon begins to taste more or less like sugared water. When the nose is released the taste becomes richer again. This method of pinching the nose is often used to get down distasteful medicine.

Another proof of this may be obtained by noting the difference in the taste of some strongly flavoured substances, cheese for instance, during expiration, when the air has been in contact with the food in the mouth, and during inspiration. The taste is much richer during expiration than during inspiration. A certain amount of concentration is necessary to observe this difference. Comparatively few people notice the difference for themselves, although everybody must have innumerable opportunities of doing so. Once it has been obtained, the experience is very definite.

No doubt, then, much of what is commonly called taste arises from the stimulation of organs in the nose. As scientists are of the opinion that these organs are the organs of smell, they are forced to maintain that a great deal of what passes for taste is really smell. Part of the rest of what ordinary people call taste arises from the stimulation of organs which physiologists regard as tactile, and temperature organs. Without proof this part is regarded as making up the entire remainder; it is maintained that, since it arises from the tactile and temperature organs, this remainder is, properly speaking, not taste.

If the current scientific way of treating taste, outlined above, be taken literally, it leads to apparent absurdities. Ice-wafers of vanilla, strawberry, or coffee-cream have the same taste; the difference, according to this way of thinking, is a difference of smell only. Warm tea with sugar in it, a piece of toffee, and all three sorts of ice-cream should taste precisely alike, they all merely taste sweet. Soup and cheese have more or less the same taste, in the direction of salt. It must by no means be thought that a bad egg tastes bad; it only smells bad. An unlimited number of instances could be adduced which show the paradoxical discrepancies between popular opinion and the standpoint of science.

These discrepancies have not made the scientists suspect their doctrine, because it often happens that the opinions of ordinary people and those of scientists differ widely, in cases where the scientists have very good reason to believe that they are right. Ordinary people believe that the earth stands still; astronomers know that the sun stands still and the earth moves. Besides, scientists have been rather cautious, as if they had some slight qualm of conscience, and they speak about what is really taste, and what in reality is not taste. It is not stated distinctly and precisely, 'this or that has nothing whatever to do with taste', but more loosely, 'in reality this is not taste', where a certain reservation is implied in this 'in reality', something to the effect that in slightly less real sense it *is* taste. It must also be remembered that if in this way different degrees of reality are introduced, what is regarded as less real is also mostly pushed aside and is not considered and so does not embarrass the scientific conscience.

The scientific doctrine which so to speak impoverishes taste has a general background in the abnegation of the sensual pleasures characteristic of the nineteenth century. It was not amiss that taste, which is so intimately connected with the pleasures of the table, which one ought to despise, was in reality restricted to the four poor qualities: sweet, sour, salt and bitter.

We shall now try to get rid of these apparent absurdities. First we shall make clear certain presuppositions and then we shall abandon them. The validity of these assumptions has been regarded so much as a matter of course that they have never been the subject of reflection, nor has it occurred to anyone to try and formulate them.

The first of these presuppositions is that there is one and only one sort of sense organ corresponding to taste, and one and only one sort corresponding to smell, just as there is supposed to be only one sort of sense organ (the eye) which corresponds to all that we see, and one sort (the ear) which corresponds to all that we hear. If this be granted the taste buds *must* be selected as the organ of taste, and the organ in the nose as the organ of smell. The next presupposition is that an investigation of taste and smell must be a study of how these organs function. Thus, the words *taste* and *smell* have acquired a new significance. In ordinary speech the word taste denotes the properties of food as they appear when the food is placed in the mouth and masticated. According to the scientific physiological way of speaking, taste is the way in which a special sense organ reacts. The apparent paradoxes arise from the fact that these two different ways of using the word *taste* are not kept distinct.

III. PROPOSAL FOR ANOTHER THEORY

We shall discard these presuppositions and return to the usages of daily life. We proceed then as scientists and say that it is characteristic of taste and smell to arise from the combined working of several sorts of sense organs. (A similar opinion could be maintained in other fields, and has been maintained for the perception of space, which has been regarded as originating from the co-operation of the sense organs of the retina and of the muscles of the eye.) The basis of our point of view is that our main interest is not to study the way the sense organs function but to study the sensuous qualities of the objects of daily life, and how they arise.

Accepting the popular meaning of the words *taste* and *smell*, we maintain that there are neither smell sense organs proper, nor taste sense organs proper. The sense organs in the nose are not *the* smell sense organs. It is true that these organs are important for smell, but they are also important for taste. According to particular conditions they subserve either smell or taste. Moreover, the sense organs in the nose are not the only sense organs subserving smell. The fact for instance that chloroform has a sweet and vinegar a sour smell is due to the co-operation of certain taste buds. In the case of some sharp and pungent smells still other sense organs function in the service of smell. In the same way the taste buds are not *the* taste sense organs. We have just seen that in some cases they co-operate in the service of smell; and the taste buds are probably in very few cases the only sense organs responsible for the taste of a foodstuff.

We propose to introduce here the two technical terms *olfactory* and *gustatory*. *Gustatory* qualities or components are those qualities or components which arise from the so-called taste buds; they may belong either to taste or to smell. *Olfactory* qualities or components are those qualities or components which arise from the organs in the nose; they also may belong either to taste or to smell. These words have been selected in order to maintain a certain, merely linguistic, continuity with the present ruling doctrine, but they must not be regarded as showing agreement with the substance of the doctrine. There are only four gustatory qualities: sweet, sour, salt and bitter, but there is an almost inexhaustible number of different olfactory qualities. No gustatory or olfactory qualities occur outside taste and smell.

Many of the olfactory qualities are common to taste and smell, and of the gustatory qualities at least sweet and sour, perhaps also bitter (and still more problematically salt). This is one main cause of the affinity between taste and smell. Whether there are olfactory qualities pertaining to smell which do not pertain to taste, and *vice versa*, has not been investigated. There are in fact practically no detailed studies of the olfactory qualities as they occur in taste. The olfactory quality pertaining to the smell of some foodstuff may be other than the one pertaining to its taste, for the process of mastication may have a certain physical and chemical influence on the food.

If our way of treating taste and smell is accepted, all the apparent paradoxes disappear. For instance, ice-cream, tea and toffee have quite different tastes; what they have in common is the gustatory quality of sweet. It is important to emphasize that the main part of the present scientific account of taste and smell stands. But it remains only as a part of the complete doctrine of taste and smell, viz. those chapters in which the gustatory and olfactory qualities are treated.

IV. OTHER SENSE ORGANS SUBSERVING TASTE

Besides the organs of the nose and the taste buds, many other sense organs subserve taste. A brief review may be of interest. The sense organs of the tongue and the mouth cavity which are concerned with touch, and which are of the same character as those of the fingers, are very important for taste. The smoothness and hardness of a piece of toffee, the soft consistency of a strawberry, and the gritty character of rice, are components of taste; components such as toughness, tenderness or crispness are also important. Later on we shall speak of juiciness and 'mealiness'.

David Katz (2) gives expression to what he himself regards as a heretical opinion: that the taste buds with their four qualities are of less importance to the taste of foodstuff than are the tactile and mechanical senses. It may be that in this respect there is a great difference between the baby and the grown-up, connected with the great difference in the food eaten. We may here refer the reader to what we have already mentioned, namely, that the papillae and with them also the taste buds are in their most highly developed state in babies, and degenerate more or less later on. Furthermore, the mimic expressions of the mouth for sweet and bitter are more characteristic of children than of grown-ups, and the baby shows the characteristic sucking reaction provoked by the quality of sweetness.

The characteristic prickling of soda-water and champagne are supposed to arise from sense organs analogous to those in the skin which, when stimulated by a fine needle may evoke an experience of prickling, in some cases amounting to pain. It is supposed that the sharp taste, produced, for instance, by pepper, arises from the stimulation of these organs.

Quite apart from the importance of the temperature for the development of the aroma in the food, the qualities of warmth and coldness may be important components of taste. One may like hot tea and dislike luke-warm tea. Cold water may be a pleasant drink, while warm water may be disgusting. In the case of ice-cream the coldness is a very important component. In addition to the temperature of the food, its chemical constitution also may give rise to the qualities of warmth and coldness. Thus peppermint and menthol induce the quality of cold: possibly also alcohol induces the quality of warmth.[1]

Further, hearing may be an element of taste, as, for instance, when one's teeth crush a biscuit or an apple. Such sounds which are conducted to the ears through the bones, and are often inaudible to other people, occur much oftener than one might suppose. As a rule these sounds are not perceived *per se*, but enter into our perception of the consistency of the food, and of the modification of this consistency during the mastication. If we say that a piece of sugar 'crunches', we are thinking just as much of the consistency of the sugar as of the sound of crunching.

Everyone considers it important for food to look appetizing, but the sense of vision may be important in other ways too. It is at any rate commonly reported (I should like to know on what evidence) that many things, for instance, red and white wine, which taste different when we are able to see them, are indistinguishable in the dark.

The foregoing remarks show that qualities from most of the known senses may enter into taste. But qualities from as yet unidentified sense organs also may come into play. There are some qualities which are observed only in the mouth cavity. Sloe, quince, tannin, etc., have a characteristic astringent quality. This quality is not experienced by the non-mucous skin. It is especially noteworthy that when a substance, such as tannin, which in the mouth shows an astringent taste, is applied to the skin there is no experience whatever of an astringent character.[1] It would also seem that the different forms of mucosity are experienced only in the mouth.

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[1] Sometimes when a taste is characterized as 'fresh' there may be a certain affinity with the quality of coldness. There is also a kind of freshness which has an affinity with sourness; it would be interesting to know whether there is a common element here. This might be studied in connection with an investigation of the whole domain: freshness-insipidity. It may be noted that for taste it has been found by Michael(3) and by Michael and Limmer(4) that the Hornbostel dimension 'Dunkelheit-Helligkeit' and the warm-cold dimension are related.

In accordance with the view that life is born in water, and that land animals must have evolved from water animals, Katz ((2), p. 56) has proposed a theory to the following effect. Water animals must have a sensorial equipment through which they are sensitive to water environment, and are warned if driven into dry regions. Among land animals this function was restricted to the mouth, which, apart from the eye, is the only 'sense organ' in watery surroundings. Katz maintains that the mouth, as a relic from our water origin, is the only organ which gives us a genuine impression of moisture and of its contrary, dryness. The tongue feels in the mouth as the fish in water. The fingers do not give us real impressions of dryness and wetness, but only a complex of touch and temperature sensations. According to Katz these sensations of wetness and dryness are very important for the water economy of our body. The sensation of dryness in our throat gives us an impulse to drink. These qualities of dryness and wetness are connected with important taste properties: juicy, mealy, dry, and the different forms of mucosity, viz. oiliness and viscosity in their different degrees (cf. water and syrup).

V. THE STRUCTURE OF THE TASTE EXPERIENCE

The richness of taste experiences is not due merely to the static co-operation of all known and unknown sense organs. A mouthful of food gives a characteristic continuously changing taste experience, not a single 'tone' but a whole 'melody'. We might speak of a taste sequence. In the case of a grape there is first the smooth, round, cool fruit. Then comes the puncturing of the skin, and the characteristic outflow of the juice. The pulp is masticated with a sucking action, and the seeds are isolated. Perhaps the skin also is chewed a little. Then follows the process of spitting out. Everyone of these phases is conjoined with a number of specific taste components.[2] If a piece of bread is masticated and mixed

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[1] This astringent quality is related to, but probably not identical with, the quality one experiences when one's eyes smart.

[2] In this connection the following extract from Brillat-Savarin may be of interest: "...je regarde comme certain que le goût donne lieu à...la sensation *directe*, la sensation *complète* et la sensation réfléchie.

"La sensation *directe* est ce premier aperçu qui naît du travail immédiat des organes de la bouche, pendant que le corps appréciable se trouve encore sur la langue antérieure.

"La sensation *complète* est celle qui se compose de ce premier aperçu et de l'impression with saliva it continuously changes in consistency, and the saliva affects the chemical constitution. This provides a continually changing taste experience. 'Melting on the tongue', *e.g.* of a succulent piece of mutton, constitutes a typical taste sequence.

A detailed investigation into the important problem of the structure of concrete taste experience belongs to the future; the sound element enters into the total experience in a way different from the astringent quality, and this again in a way different from the quality of sweetness.

It will be seen then that what we call taste may roughly be said to consist of properties of food (or other substances) as they are experienced mostly during, but sometimes before, their treatment in the mouth. This treatment is many-sided and may be characterized by words like biting, mastication, insalivation, swallowing, and occasionally elimination of undesirable particles such as seeds, pieces of bones, small stones, hairs, etc. (Cf. Brillat-Savarin for a description of the movements of the tongue ((5), p. 31).) The treatment is somewhat less complicated with fluid than with solid food, and is less complicated in societies where what we call polite manners prevail (where, for instance, a knife and fork are used instead of an initial biting off of a mouthful of food). According to Höffding a famous Shah of Persia remonstrated with the Europeans because they did not know that taste begins at the finger tips.

The little world of the mouth cavity constitutes, according to William James, a kind of specific space which is not made to form part of ordinary space experiences. For instance, the notions which the tongue gives us of the size of a hole in a tooth, and of the movements of a tooth which is loose, are absurdly exaggerated, if compared with what vision would tell us. It is therefore possible that taste properties which are conditioned by such sense organs as appear also outside the mouth cavity and the nose, have or may have a special 'timbre', a specific character. In many cases experience seems to point in this direction, but not in all cases: the smoothness which the tongue may experience with a piece of toffee may,

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qui naît quand l'aliment...passe dans l'arrière-bouche, et frappe toute l'organe par son goût et par son parfum.

"Enfin, la sensation *réfléchie* est le jugement que porte l'âme sur les impressions qui lui sont transmises par l'organe....

"Celui qui mange une pêche, par example, est d'abord frappé agréablement par l'odeur qui en émane; il la met dans sa bouche, et éprouve une sensation de fraîcheur et d'acidité qui l'engage à continuer; mais ce n'est qu'au moment où il avale et que la bouchée passe sous la fosse nasale que le parfum lui est révélé; ce qui complète la sensation que doit causer une pêche. Enfin, ce n'est que lorsqu'il a avalé que, jugeant ce qu'il vient d'éprouver, il se dit à lui même: 'Voilà qui est délicieux!'" ((5), pp. 27–8).

for instance, be quite analogous to the feeling of smoothness transmitted by the finger tips.

That the tongue can sometimes be used as an organ of touch without any connection with taste constitutes a difficulty in the demarcation of the domain of taste; another arises from the uncertainty as to whether some experiences in the mouth cavity belong to the food proper or to something connected with the food or the treatment of it, *e.g.* with the flow of saliva which the food occasions.

On the whole it will be seen that what we understand by taste is something of indefinite delimitation. In contrast with this the concept of taste, according to the current scientific doctrine, has very clearly drawn boundaries, but this is achieved by keeping to a very small and arbitrarily limited domain. The larger and more difficult, but vaguer, domain remains the same, but is not made the subject of scientific treatment.

There are special conditions in the attitude of civilized humanity towards food which place obstacles in the way of the scientific treatment of taste. We relish good fare, but when we eat we do not really taste; if we are in company we are occupied in conversation; alone we may read a paper, or our thoughts may be occupied with things far away from the taste of what we are eating. So perhaps for us the most important rôle of taste is to warn us of anything unusual. In such cases we often speak about a queer taste. Probably if the taste did concern us more while we eat, our taste experiences would be better guides to what is the most wholesome food for us.

This habitual 'lack of attention' makes it difficult to collect experiences of taste for scientific treatment. We have to conquer our inveterate habit of being occupied with something else. And should we ourselves overcome this customary attitude, our taste experiences would become so rich and differentiated that they could not be wholly verified by other people. Again, if a subject has had insufficient psychological training, his attention may be diverted, without his becoming aware of it, into unsuitable channels, so that something corresponding to what is ordinarily only one side of the total experience becomes isolated, whereby important properties may be lost. If, for instance, one becomes interested in the sound that enters into taste, it may happen that one experiences only isolated sounds which have lost their character of being subordinate indicators of the consistency of the food.

VI. EVIDENCE FROM EXPERT TASTERS

Were one to ask taste experts from different fields whether they would prefer the current scientific way or our way of treating taste, there could be little doubt that we should find that the former would not suit their needs.

In a letter to the author in which an expert writes expressly on the taste values of chocolate, he speaks of "the sight of perfect gloss, a consistency giving a fine bite, the smooth feeling when chocolate fondant passes over the tongue and the palate", and he says that "the taste of chocolate tempered in a wrong manner giving it a consistency like rye bread is altogether different from that of the same size and quality of chocolate correctly cooled down, which has a glass-hard break".

The Danish scientific agriculturalist, Mr Frode Hansen, has written an interesting report on the taste of potatoes and its dependence on the manure used (6). In some brief remarks he speaks about the confusion which arises from the difference between the common usage of the word taste and that of psychology and physiology. His mode of argument and his proposals for a terminology have a certain affinity with our treatment.

With regard to taste experts we may perhaps remark that there may be a great difference between what may be called analytic experience, having a background of greater or less knowledge in a special taste field, and the more synthetic experience of the ordinary person. For such special purposes as the buying, blending, and manufacturing of foodstuffs, the observations of the trained food expert are important, on account of his power of analyzing and describing details, etc., and his knowledge of the causes determining special properties. But in ascertaining the likes and dislikes of the ordinary consumer it would probably be better to have recourse to a methodical collection of the experiences of ordinary people.

Finally, when we consider taste in its general biological aspect of being connected with appetite, selection of food, and digestion, it is evident that it is taste in our sense, and not in the accepted scientific sense, that is involved. What is of importance is taste as a complex whole experience, not merely taste in the rather arbitrarily restricted sense of the four gustatory qualities.

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THE VARIATION OF SENSORY THRESHOLDS WITH THE RATE OF APPLICATION OF THE STIMULUS

I. THE DIFFERENTIAL THRESHOLD FOR PRESSURE

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I. Introduction

In making careful measurements of sensory thresholds it is usual to control the rate at which the stimulus is applied, and there is evidence that the value of the threshold is, in some cases, a function of this rate. For instance, Rawdon-Smith and Grindley (1935) have recently described an illusion in the perception of loudness which indicates that the differential threshold for auditory intensity is greater when the intensity of a sound is changed slowly than when it is changed rapidly.

A number of investigations, designed to measure the variation of thresholds (both absolute and differential) in different sensory modes with the rate of application of the stimulus, are now in progress at the Cambridge Psychological Laboratory. The results of such measurements may be expected to be relevant to the discussion of problems about the measurement of sensation, Weber's law, and sensory adaptation. The differential thresholds under investigation are, of course, *successive* thresholds, such as the just perceptible difference when the brightness of a light changes, rather than simultaneous thresholds, such as the just perceptible difference in brightness between two simultaneously presented lights.

The present paper describes measurements of the variation of the differential threshold for pressure with the rate of change of the pressure. The pressure was applied to an area of about 1·8 sq. cm. on the back of the hand, and the total pressure applied to this area ranged, in different experiments, from about 200 g. to about 750 g. The rates of change of pressure were such that a just perceptible increment occupied about 0·005 sec. in the most rapid changes employed, and about 2·0 sec. in the slowest.

II. APPARATUS

The apparatus is shown diagrammatically in Fig. 1. The subject's hand rested, palm downwards, on a platform P, the height of which could be regulated by means of a levelling screw. The pressure was applied through a projection P' below a wooden arm A_1, which latter was pivoted on point bearings at one end. The arm A_1 was 35 cm. long, and weighed 50 g., and the projection P' was 17 cm. from the pivot. The bottom of the projection was covered with a circular patch of fibre, 1·5 cm. in diameter, to ensure more even distribution of the pressure than would be given by a bare wooden end. The position of the arm A_1 could be read from a scale S_1, and it was kept at a fixed position during experiments by adjusting the levelling screw when necessary. The pressure applied to the hand was determined by the tensions of three elastic bands E_1, E_2, and E_3; and it could be measured by means of a balanced arm B, from one end of which was hung a scale pan. Except when it was in use for calibrating the apparatus, the arm B was kept tilted so that the string S was slack.

The tension on the band E_1 was kept constant during any one experiment, and variations of pressure were introduced by changes in the length of E_2 (for rapid changes) and of E_3 (for slow changes). The lower end of E_2 was attached to a light aluminium lever A_2, pivoted on point bearings at one end, and working up and down between adjustable rubber-covered stops RR. The position of the lever could be read from a scale S_2. A_2 was attached also, by means of an elastic band E_4, to a pendulum D, which had a heavy iron bob, and a natural period of about 1·2 sec. The position of the pendulum could be read from a scale S_4. A swing of the pendulum (with sufficient amplitude) caused the lever A_2 to move from one stop to the other at an approximately uniform speed. The change in pressure at the subject's hand was therefore determined in extent by the distance between the stops RR, and in rate by the amplitude of the swing of the pendulum D. The pendulum was operated by hand, the experimenter moving it to any required position, allowing it to make one half swing, and then returning it to the required position.

The top end of the elastic band E_3 was attached to one end of a wooden arm A_3, pivoted in the middle. The extent of the downward movement of this end of the arm A_3 was determined by an adjustable stop S' above the other end. The position of the arm could be read from a scale S_3. Between the stop and the pivot, a chain attached a point on the arm A_3 to a point vertically below it on a similar arm A_4, as shown in Fig. 1. The other end of A_4 had a steel projection which worked on a cam C. This cam was of $\frac{1}{8}$ in. brass and was so cut that for successive quarters of a revolution it caused the end of the arm A_4 to rise uniformly, to remain still, to fall uniformly, to remain still,

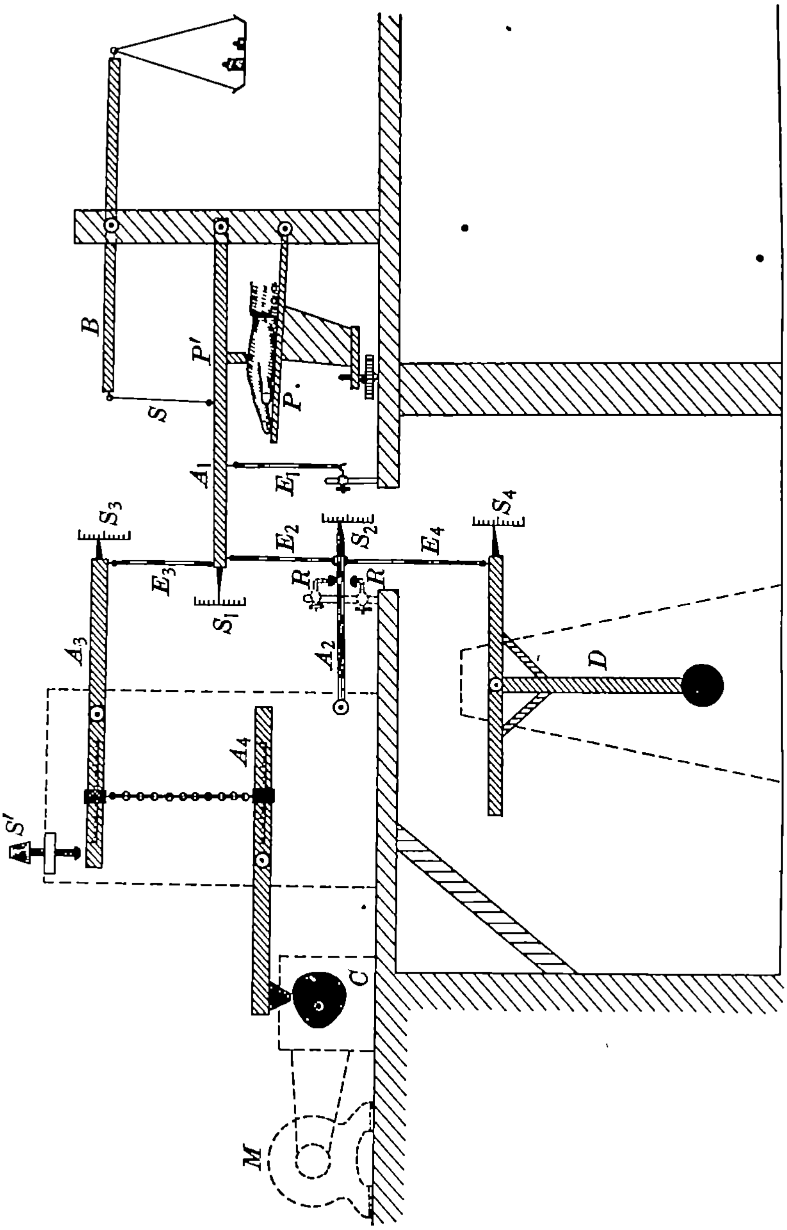

Fig. 1. Diagram of apparatus.

and so on, the extent of the rise and fall being 1 in. The cam was driven, through a reducing gear and a system of pulleys, by an electric motor *M*, at speeds ranging from 3 to 30 r.p.m. The motor *M* was on a window still, and the arms A_3 and A_4 were mounted on a shelf fixed to the wall (as shown in Fig. 1), in order to minimize the transmission of vibration from the motor to the platform upon which the subject's hand rested.

The points of attachment of the chain to the arms A_3 and A_4 were variable (the point of attachment to A_3 being always vertically above the point of attachment to A_4), and this arrangement made possible a wide variation in the ratio in the speed of movement of A_3 to that of A_4—and hence, for a given speed of rotation of the cam, a wide variation in the rate of change of pressure at the subject's hand. But in practice it was found better to vary the rate of change of pressure at the subject's hand by varying the speed of rotation of the cam, rather than by adjusting the position of the chain, since if the chain was placed close to the pivot of A_3 (so as to give a high gear ratio between the two arms) the arm A_3 oscillated appreciably during its movement. The pressure change at the subject's hand was therefore determined in extent by the position of the stop *S′*, and in rate by the speed of rotation of the cam *C*.

III. CALIBRATION AND LIMITS OF ACCURACY OF THE APPARATUS

Calibration of the pressure exerted on the subject's hand for various positions of the levers A_2 and A_3 was a simple matter; since (with the subject's hand removed) the pressure could be calculated from the weight which had to be placed in the scale pan hung from *B* in order to bring the lever A_1 to its zero position on the scale S_1. Such calibration was done several times each day because it was found that the tensions of the elastic bands varied slightly from time to time (*e.g.* with changes of temperature).

The rate of change of pressure due to movement of the pendulum or to rotation of the cam could be calculated from the physical dimensions and properties of the apparatus; but it was, in fact, determined more directly by attaching light celluloid points to the arms A_2 and A_3, and recording their movements on a kymograph. This method made possible the detection and measurement of any oscillations or other deviations from uniform motion. Threshold determinations were made only at speeds at which such deviations were found to be small. It was found that the lever A_3 worked satisfactorily at speeds of rotation of the cam of from 3 to 30 r.p.m.—corresponding to rates of change of pressure of from 100 g. per 2·0 sec. to 100 g. per 0·2 sec. At higher speeds oscillations ceased to be negligible, and the pendulum was therefore used. This gave satisfactory movements of the lever A_2 at speeds corresponding to rates of change of pressure of from 100 g. per 0·14 sec. to 100 g. per 0·05 sec.; but it is unlikely that the pressure at the subject's hand followed the movement of A_2 accurately at the highest of these speeds. The arm A_1 weighed 50 g., and the movement of the end of the arm (observed microscopically) for a pressure change of 10 g. was of the order of 0·015 mm. Calculation suggests that the inertia of A_1 would not introduce any appreciable error except at the highest speeds; but the extent of the error at these high speeds could not be calculated owing to lack of information about the relevant elastic and viscous properties of the hand. The movement of the arm A_1

was therefore magnified mechanically by means of a light aluminium lever, recording on a kymograph. This arrangement could not be made sensitive enough to take an accurate record of the movement of A_1 when the pressure changes were only of threshold value; but the records suggested that at the highest speeds of A_2 the movement of A_1 was somewhat slower (perhaps 50 per cent.) than its calculated value. The records also showed that the sudden movement of A_1 was not followed by any large oscillations. In Figs. 2 and 3 the values given for the rate of change of pressure are calculated from the movement of A_2; but it should be remembered that the actual rates of change (at the highest speeds) are probably appreciably slower than those shown, and it is also possible that these rapid changes may have initiated oscillations which might be significant in lowering the apparent value of the threshold.

Two other ways in which the design of the apparatus may have introduced inaccuracies into the threshold determinations must be mentioned. The apparatus was not silent in operation, and the noise from it gave the subject an auditory cue as to when to expect a change of pressure, and, since the pendulum-driven apparatus and the motor-driven apparatus made noises of different kinds, the auditory cue was different at different rates of change of pressure. (In order to minimize the influence of this, the experimenter said 'Ready' just before each change.)

It will be seen also that, using variations in the rate of rotation of the cam to produce variations in the rate of change of pressure, the interval between the changes of pressure was a function of the rate of change of pressure. It is more desirable, perhaps, that the interval between stimuli should be constant; but that the variation in the interval did not introduce any serious errors under the conditions of this particular experiment was suggested by several control tests with the pendulum, in which the interval between stimuli was varied over a wide range (corresponding to the range of intervals due to the variations in the speed of the cam) without any appreciable effect on the threshold.

IV. EXPERIMENTAL PROCEDURE

The subject was seated, with his elbow on a rest, and his hand palm downwards on the platform P. The projection P' from the arm A_1 rested on the back of his hand, about 1 in. proximal to the knuckle of the middle finger. The position of P' was marked in ink, so that it could be replaced if the subject removed his hand, for a rest, during the experiment. (Rest pauses were allowed at intervals, to prevent discomfort from the hand and arm.) The instructions were: "When I say 'ready' you may feel an increase (or decrease) of the pressure on your hand. If you do so, report it."

With the arm A_1 set at its zero position by adjustment of the levelling screw, and the apparatus set so as to give any required rate of change of pressure, the threshold for that rate of change was determined by the limiting method, the stop (S' or R) being set at first so that the change was easily supraliminal, moved until the change became subliminal, returned to a supraliminal position, and finally to a subliminal position. Further readings were taken if these four initial readings showed a wide scatter. The threshold was then determined in the same way for other rates of change of pressure, and at the end of a series of readings the threshold at the rate of change used in the first reading was again determined, as a control. The rates of change were taken in different orders in different experiments.

At the end of the experiment the subject was asked to describe his sensations both in the fast and in the slow changes, with special reference to the localization of the sensations, and was given a few supraliminal changes of both kinds for observation from this point of view.

V. QUANTITATIVE RESULTS

Fig. 2 shows the results obtained in four experiments with different subjects with a standard pressure of 200 g. The threshold values (plotted as ordinates) are the just perceptible *increases* of pressure. The abscissae represent the times required for a given change of pressure (*i.e.* the reciprocals of the rates of change of pressure). Fig. 3 shows four similar curves with a standard pressure of 500 g. Sixteen such curves were obtained, with standard pressures ranging from 200 to 750 g.

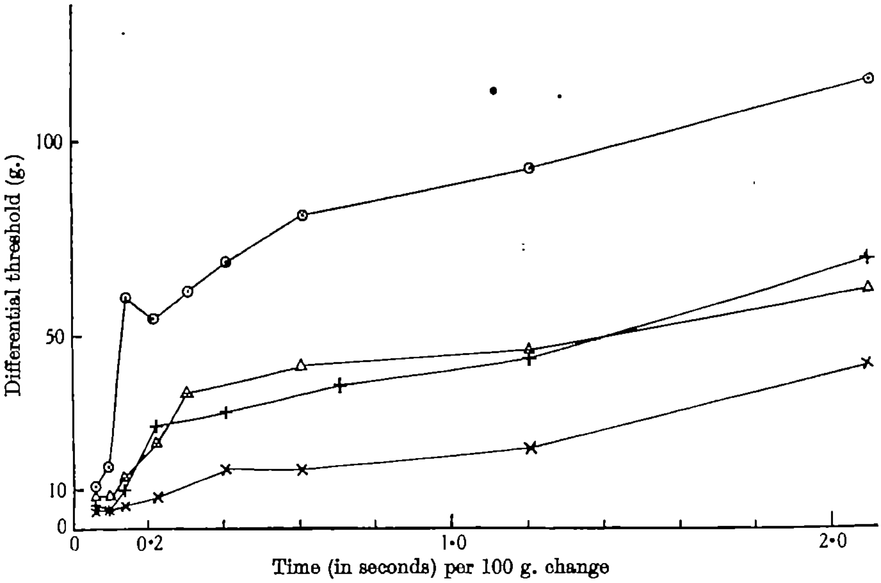

Fig. 2. Differential threshold for increase of pressure. Standard pressure = 200 g.
⊙ = subject A.F.R.S. + = subject E.G.C.
△ = subject J.M.B. × = subject M.D.V.

All the curves showed a large, and progressive, increase in the threshold with increase of the time occupied by a given change of pressure. At the slowest changes (about 2·0 sec. per 100 g.) the threshold was usually at least ten times the threshold at the fastest changes (about 0·1 sec. per 100 g.).

No great importance is to be attached to the exact form of the curves, owing to the limitations in the accuracy of the apparatus which have already been discussed (in section III). But it is worth pointing out that for the slower changes (above 0·2 sec. per 100 g.) all the curves approximate to straight lines which, if produced, would cut the ordinate axis above the origin. As the curves approach the origin (below about 0·2 sec. per 100 g.) they appear to fall more rapidly, and, finally, the last two points (0·09 and 0·06 sec. per 100 g.) usually show a flattening out of the curves. It must be remembered, however, that the values of these rapid rates of change are uncertain (cf. section III) and also that the three fastest rates were obtained with the pendulum-driven apparatus, whereas the slower rates were obtained with the cam-driven apparatus, and that there is a possibility that there may have been significant vibrations at the highest rates.

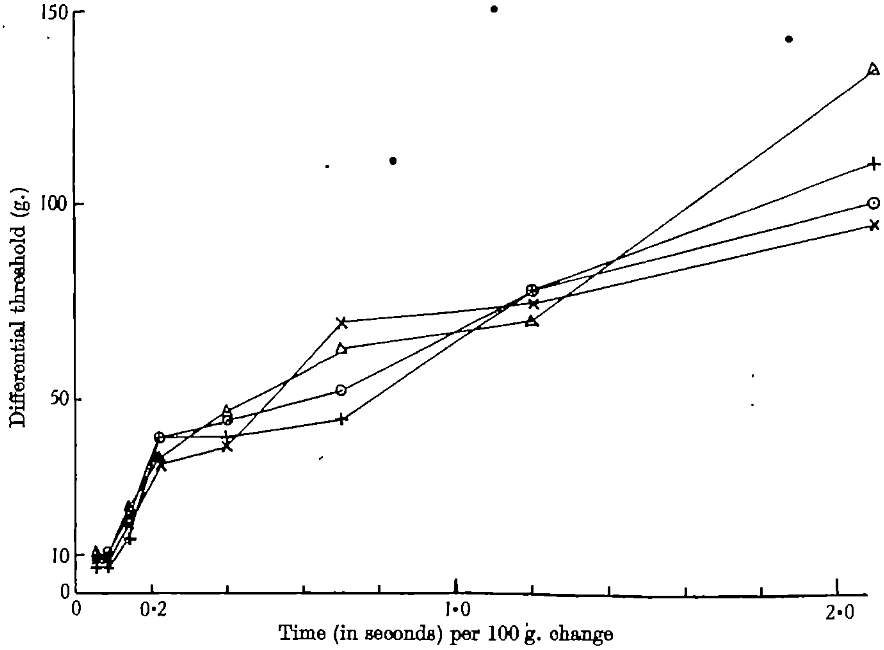

Fig. 3. Differential threshold for increase of pressure. Standard pressure = 500 g.
△ = subject R.C.O. ⊙ = subject A.C.
+ = subject E.G.C. × = subject M.D.V.

The threshold for *decrease* of pressure usually appeared to be considerably greater (*e.g.* 50 per cent. greater) than the corresponding threshold for increase of pressure. This made it difficult to determine the threshold for decrease of pressure with accuracy, since the subjects reported that they were continually distracted from, and influenced in, their judgments about barely perceptible decreases of pressure by the much more easily perceptible increases which alternated with them. The threshold values showed such a wide scatter that the only conclusions which could be drawn were (*a*) that the threshold for decrease of pressure is usually greater than that for increase of pressure, and (*b*) that the threshold for decrease of pressure increases with increase in the time for a given change.

VI. SUBJECTS' REPORTS

All the subjects agreed that rapid changes of pressure felt qualitatively different from slow changes. One subject described a sudden change as "a jerk to the hand", and a slower change as "a gradual squeeze". Several subjects reported that whereas with the rapid changes they "felt the change", with the slowest changes they "judged that the pressure was greater (or less) than it had been". Another type of report, for the slowest changes, was that (some time after the pressure had actually begun to increase) the subject "suddenly became conscious that the pressure was increasing gradually".

The sensations upon which the appreciation of change was based were variously localized as originating in the cutaneous area under the pad, deeper tissues under the pad, adjacent superficial or deeper regions, the palm of the hand (so that an increase of pressure was felt as a push upwards by the platform upon which the hand rested rather than as a push downwards by the pad), or in some combination of these.

VII. DISCUSSION

The fact that the differential threshold for pressure varies so markedly with the rate of change of pressure emphasizes the danger of speaking of a sensory threshold as though it were a fixed quantity, defined sufficiently by referring to its sensory mode and its place of origin. It is obvious that the value of an absolute threshold is a function of the state of adaptation of the receptor, and certain recent work at this laboratory (unpublished) suggests that the same is true of differential thresholds. It may be possible, and useful, to treat just perceptible increments as 'units' in constructing a 'scale of sensations', and to frame laws, of the general type of Weber's law, connecting the differential threshold with the intensity of the stimulus; but it is clear that in either case the state of adaptation, and (for successive thresholds) the rate of change of the stimulus must be specified. If, for instance, *A*, *B* and *C* are three intensities of stimulation, such that under one set of conditions the number of just perceptible increments in passing from *A* to *B* is the same as that from *B* to *C*—there is, at present, no evidence to warrant the assumption that this relation will also hold under other conditions of adaptation or of rate of change of stimulus.

The phenomenon described in this paper may be explained in terms of sensory adaptation. If the perception of an increase of pressure depends upon an increase in the number of receptors stimulated and nerve fibres active, the thresholds for individual receptors being different, and if the adaptation time of the receptors is less than the longest time occupied by a just perceptible change, then it is clear that more fibres will be responding simultaneously during a rapid increase of pressure than at any time during a slow increase. If the perception depends on the frequency of impulse in individual fibres, and if this frequency increases with the intensity of the stimulus and decreases with the time since the stimulus was applied (cf. Adrian, 1932), then it is clear that a rapid increase of pressure will give rise—momentarily—to a higher frequency of impulses than will ever be produced by a slow increase of pressure. On either view, or if the perception depends upon a combination of these effects, it is to be expected that the curve showing the variation of a sensory threshold with the rate of application of the stimulus will be steeper, for rapid changes, for sense organs which adapt rapidly than for those which adapt slowly. Anatomical and physiological considerations, as well as the reports of the subjects in this experiment, suggest that the sense of pressure involves a wide variety of receptors, but the relative importance of different kinds of receptor is not known. Comparison between the slopes of the curves obtained in this experiment and the physiological data on the rates of adaptation of the receptors involved is, therefore, not possible.

It is possible, of course, that the phenomenon described in this paper depends upon a central process of adaptation as well as upon peripheral mechanisms; but the evidence does not make this assumption necessary. The subjects' introspections suggested, however, that the central processes leading up to a report that the pressure had increased were sometimes radically different for the slowest changes and for faster changes. For fast changes the subjects agreed that the change was directly perceived, and it may be suggested that the peripheral basis of such a direct perception of change lies in the 'on effect'—*i.e.* the burst of nervous impulses which follows the sudden application, or increase, of a stimulus, but which dies away as adaptation ensues. For the slowest changes the subjects sometimes reported that they did not perceive the change directly but that they judged, for instance, that the pressure was now greater than it had been a few moments previously. It may be suggested that the peripheral basis of such comparative judgments lies in the difference of levels of stimulation before and after the change, rather than in any nervous events initiated by the change itself. If this is the case, it may be found that the threshold for very slow changes is influenced by psychological factors such as 'time errors' (cf. Koffka, 1935) which have little or no effect on the threshold for faster changes.

VIII. SUMMARY

Evidence is given that the differential threshold for pressure on the hand increases progressively as the rate of change of pressure is decreased. With standard pressures of from 200 to 750 g. applied to an area of 1·8 sq. cm. on the back of the hand, the threshold for an increase of pressure at a rate of 50 g. per sec. may be more than ten times as great as the threshold for an increase of pressure at the rate of 2000 g. per second. The threshold for a decrease of pressure is usually greater than that for an increase at a similar speed.

It is pointed out that these results can be explained in terms of sensory adaptation.

I wish to express my thanks to Prof. Bartlett for his advice, and to the Medical Research Council for a personal grant.

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CRITICAL NOTICE

By O. A. OESER

(*From the Psychological Laboratory, St Andrews*)

Principles of Gestalt Psychology. By K. Koffka. London: Kegân Paul, Trench, Trubner and Co., Ltd. 1935. Pp. xi+720. 25*s*. net.

For many years the psychological world has been waiting for a systematic treatise on the experimental principles of Gestalt psychology. The accumulation of special papers has grown so as to be unmanageable for the research worker, harassed by the demands of teaching and tossed about in the floods of literature in his own language. Köhler's outline in 1929 whetted the appetite; Koffka's book now sets before us the products of his own indefatigable researches, and the fruits of his careful selection from the works of the new school. There can be no doubt that this book will have a great influence on psychology, for when scattered results are co-ordinated and systematized, they can begin to influence the future of a science far more rapidly and completely. But for the systematization of Wundt and Titchener, it is doubtful whether atomisms could have dominated theoretical psychology for so long, in spite of their manifest inability to explain fundamental facts like the constancies, or to make intelligible the organic unities of human behaviour.

Koffka's book, too, comes at the right time. Atomism has been almost everywhere transcended, and is being seen in its proper place and perspective, in the work of brain physiologists like Lashley, of neo-behaviourists so diverse as Skinner, Hull and Tolman, and in the work of Bartlett on Memory, quite apart from the adherents of the Gestalt School proper. Had James Ward and G. F. Stout been experimental enquirers, we need perhaps not have waited so long for a book like Koffka's. Stout saw the futility of associationism and sensationism forty years ago, and showed that the 'experience error', so ably exposed later by Köhler and Koffka, involved a circular argument. But Stout wrote in the heyday of Wundt. He had no laboratory with 'brass instruments'. So, although he delimited the fields of psychology, philosophy and metaphysics in the most lucid prose and with compelling logic, his *Manual* had but little effect on the development of experimental psychology. Now that Koffka's volume is before us, we can see that, in some fundamental respects, Stout's logical analysis has been verified by experiment, and so once again link psychology to the best traditions of English *empirical* philosophy.

Again, in its general anti-sensationistic outlook, in its insistence that the biological and psychological sciences cannot overlook the integrative properties of various processes, Gestalt theory is supported by the functionalist school of physiologists like Sherrington. Koffka's use of 'Gestalten' has resemblances to Head's and Bartlett's 'schemata' and MacCurdy's 'patterns'. There is, therefore, overwhelming historical evidence that 'sensations'—and all that that term implied—are dead. I do not think that the resemblances go much deeper than the agreement, in principle, to discard the sensationistic hypothesis. Gestalten are supposed somehow to provide causal explanations; patterns and schemata are used more for descriptive purposes. Koffka tries to show that Head's term 'vigilance' is to be interpreted as 'energy' or 'available energy', a view strongly opposed by Bartlett and, I believe, by Head himself.[1]

Principles of Gestalt Psychology is a very long and difficult book. And so it ought to be, for we cannot hope to fathom psychology in a day or describe it in a sonnet. The book's length and richness must serve as an excuse for the inadequacies and sketchiness of this review. The reviewer can do no more than say: "I have read this; go and do thou likewise"; and try to give some indication of the results of its impact on one reader. It is a systematic presentation of hundreds of special papers, and it will need hundreds more to enlarge its scope or refute its teaching.

The book falls, broadly, into five parts:

(1) 'Foundations', the task of psychology, theoretical concepts (chaps. I–III).
(2) Perception (chaps. IV–VII).
(3) Action—reflexes, emotions, attitudes, the will (chaps. VIII–IX).
(4) Memory and learning (chaps. X–XIII).
(5) Society and personality; conclusion (chaps. XIV–XV).

In the preface Koffka insists that he has no wish to be dogmatic, and that his book must be judged not only as *Gestalt* psychology but as a psychology. He wrote it only after he had answered for himself the question "what is it that a student of psychology should be able to gain from his general course,...that psychology can contribute to the imperishable possessions of our race" (p. 4). Let me say at once tl scrupulously fair to his opponents and anxious to show that necessarily still exist in the system and its experimental proofs the general student of psychology, the chapters on perceptior supply him with a model of analysis, of exposition, and of the use of simple examples to illustrate a point. He may be a little by references to 'our psychology' (*e.g.* pp. 67, 333) after what said in his introduction; he will certainly find it a hard nut to cra if he has already had several general courses. But then it is ab that psychology students were led to do some hard thinking ir strolling bucolically through the various fields of psychology, c odd facts as the amateur botanist collects leaves of grass. In rece there have been too many 'text-books' addressed to both "the layman and the academic student"; and I for one am glad tha has failed to produce what he originally set out to: "a boc would have a position intermediate between Köhler's 'Gestalt logy' and an ordinary text-book"—a failure for which he need apologized (Preface).

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[1] *Vide* F. C. Bartlett, *Remembering*, Cambridge University Press, 1932, and his review of Head's work on Aphasia in *Brain* (1926), XLIX, 581.

In the first part, Koffka distinguishes carefully between c ness and consistency. He points out that because Gestalt psyc a new approach, there can as yet be no complete treatment of al aspects of this science. "By its demand for exactness science fı from the personal wishes of the scientist. A theory must be d by facts; in its turn it demands facts, and if they fail to confor to it, then the theory is either wrong or incomplete. In this sen is discipline" (p. 9). It is therefore far more important tha known should be treated in such a way that it falls into a c theoretical framework. If consistency is aimed at, comple exhaustiveness is bound to come in the course of time. With may agree all the more readily when one sees how often syst from the outset aim at completeness force *a posteriori* a super sistency on their experimental proofs. I am inclined to thi reading Koffka's book, that this is happening to Gestalt psycho perhaps I am wrong, or it is as yet too early to make such a j At any rate I shall try in what follows to show why I thin review cannot concern itself with an analysis of the experime presented by Koffka. That must be left to special papers by a competent to judge. All I can do is to review some aspects of retical position.

Koffka sees psychology as the science in which the scien animate nature, life and mind meet. Their focus lies in the central nervous system whose structure is ultimately physico-chemical, whose function is like the function of all living organisms, and whose behaviour includes the behaviour known as consciousness. Now "psychology deals with the behaviour of living beings. Therefore, as every biological science, it is faced with the problem of the relation between animate and inanimate nature whether it is aware of and concerned with this problem or not" (p. 10). He therefore passes under review various solutions of this problem, rejects materialism and all forms of vitalism, and arrives at a solution by extracting from each of the sciences of Nature, Life and Mind "one specific and important concept, viz. from the first: quantity; from the second: order; and from the third: meaning or significance (in German: *Sinn*)" (p. 13). He sees that quantity—quality is not a true antithesis at all. Such a misconception arises only when we neglect the *distribution* and *relation* of the quantities. This relation is a qualitative aspect, and no less factual than the quantitative aspect itself. Therefore psychology "may be perfectly quantitative without losing its character as a qualitative science, and...may be unblushingly qualitative, knowing that if its qualitative descriptions are correct, it will some time be possible to translate them into quantitative terms" (p. 15). As for order, the solution of materialism and vitalism having been rejected, Koffka seeks to demonstrate throughout the book that order is not only a characteristic of Life, but of Nature as well. Later, he uses the word 'organization', sometimes instead of order, and sometimes for the processes that produce order. The chapters on perception show that order (organization) is present from the beginning in a perceptual field and is a condition of perception, not a result of it.

"What we mean by significance and value is harder to explain than the two previous concepts, yet here lies one of the deepest roots of Gestalt theory, one which has been least openly brought before the English-speaking public" (p. 17). The reason, Koffka thinks, is to be found in the different intellectual climates of German and Anglo-Saxon culture. In Germany, the historian, for example, "is often more interested in the relation of a great man to the plan of the universe than in his relation to the events on the planet" (p. 18). In America, the practical exigencies of the moment hold the stage, metaphysics is relegated to the useless and non-existing, science and philosophy tend to positivism. When Gestalt psychology was first presented in English, therefore, its factual discoveries were stressed and its metaphysical implications kept in the background. After discussing the dilemma of German psychology

—causal analysis versus *Geisteswissenschaft*—Koffka shows that the Gestalt principle achieves a resolution of this dilemma by synthesis. That is, on the one hand he uses the order (organization) of physiological systems in dynamic equilibrium as a causal principle in perception, and on the other uses the qualitative descriptions (introspections) of direct experience (consciousness) to point to a problem and throw light on the "underlying physiological dynamic context". Put more simply, he accepts the traditional scientific method of explanation by causal analysis and, at the same time, reintroduces 'meaning' by giving it a psychophysical instead of a spiritual connotation. Koffka ends the chapter by expressing the hope that "Psychology...can lay the foundations of a system of knowledge that will contain the behaviour of a single atom as well as that of an amoeba, a white rat, a chimpanzee, and a human being..." (p. 23).

This summary of a preliminary chapter may seem unduly long. But we have to see how saturated with metaphysics Koffka is before we can assess the further systematic theoretical treatment of the experimental data he expounds. He gives the reason for this himself: coming from the intellectual climate of Germany he was bound to be saturated with metaphysics. If, in the discussion that follows, I seem to be less so, I must plead in extenuation a different cultural background, and leave the reader to assess, if he thinks it possible and desirable, the relative merits of two different views as to the scope and methods of theoretical psychology.

Koffka distinguishes between 'simple facts' and 'complex facts'. He insists that the latter must be derived from the former and not *vice versa* (pp. 4–5), and that psychology, like all other sciences, must seek simple facts. But he does not, except perhaps implicitly, make the further point that what are 'simple' facts must be specially determined by observation and experiment for each field of investigation. For instance, derived or secondary desires are simple facts for social psychology; for to break up the desire for beer which assails one in a German restaurant into the primary tissue need for water and various unspecified conditioned responses, is to lose precisely the fact of the social setting of the desire. At the individual level, to break up thirst into tissue needs for H_2O and various glandular or pharyngeal processes is to pass over into physiology, and in so doing to lose those qualities that make thirst also a legitimate study for the *psychologist*.

In view of what will be said later about the lack of operational definitions in this book, this point seems to me to be important. I hold that it is essential to have the means of distinguishing between the fields of psychology and physiology. Köhler and Koffka do not, for they place Wertheimer's principle of the isomorphism of psychological and physiological processes at the very centre of their systems. By turning to physiological facts for their ultimate explanation, and attributing to these physiological facts the systematic formal properties of 'mind' or 'direct experience' (and *vice versa*), they make the body-mind problem disappear by showing it to be ubiquitous (p. 62).

But it is arguable that the 'body-mind' ('inanimate'-'animate' nature) problem is not germane to the theory of experimental psychology at all—in fact most experimental psychologists, and scientists in general, implicitly or explicitly act on this assumption. "As scientists", they would say, "we are not entitled to the luxury of metaphysical speculations, though as individuals we may be." The metaphysical speculations of Jeans and Eddington do not advance theoretical astronomy any more than Koffka's metaphysical speculations advance theoretical psychology.

If we wish to make psychology scientific, we must conform to the accepted and proved principles of scientific procedure. The very first step to take is to escape from philosophy. In physics, this step was taken when Galileo started systematic experimental mechanics. How does the physicist define terms like 'temperature' and 'force'? If he is talking to a layman he will say vaguely: "temperature is the state of hotness or coldness of a body." But if he is in the laboratory, or talking to a trained scientist, he says: "the temperature of a body is defined by the height to which a column of mercury in a standard glass tube rises when the tube is brought into contact with the body under observation"; "a unit of force is defined by the work done in moving a weight of one gramme through a distance of one centimetre". A science begins, then, when someone makes observations. Things are hot, they fall from heights, and so on. This is the *descriptive* stage. When description is systematized, we have the *experimental* stage. But the systematization of description depends on the introduction of a *theoretical* stage. The symbols used in a theoretical science are symbols like T. (temperature), F. (force), which are defined through operations, as above. Note that I say 'a' theoretical science and not 'theoretical science'. The latter term has no meaning, but is used in two ways. Sometimes it is used to mean the theory of logic as applied to the practice of logic, which, since we must use language, must underlie our operations with symbols. More often, however, it is used to mean 'speculative science', that is, philosophy and metaphysics combined. A theoretical science like physics has been made consistent, it seems to me, because its definitions are operational just as much as because it has learnt to control the conditions of an experiment.

Let me sum up by quoting from Stout, *Manual of Psychology*, 3rd ed., p. 78: "From the merely scientific point of view, we have to determine what is the most likely hypothesis which can be gathered from interpretation and generalisation of the observed facts. Metaphysics finds that any view reached in this way involves difficulties and problems which require for their solution some theory concerning the ultimate nature of matter and mind and their relative place and function within the universe as a whole." *Speculative* science, then, may be concerned with the analysis of verbal fictions like object, real, mind, matter; a *theoretical* science is concerned only with the legitimate generalizations of the empirically observed facts in its particular sphere. These 'facts' are constructs, no doubt, but *they are defined by means of operations and their effects and not by* a priori *acts of intuition.*

It has already been said that the concept of isomorphism of physical (physiological) and psychological structures is basic to Gestalt psychology. This alone is not enough. The various terms must be more accurately defined by means of the operations through which we know them.[1] However, once we accept isomorphism, we can accept further principles. Whenever dynamic physical systems are in a stationary (time-independent) state, they will conform to general laws: the energy output will tend to a minimum, and other properties, such as volume, will tend to give maximum or minimum distributions. The surface of a soap bubble, for instance, is a sphere, which is the maximum surface possible under the given conditions.

Again, in self-adjusting systems, the whole is more than the sum of its parts, and can be transposed into different systems of co-ordinates or described by other parameters. These are the well-known von Ehrenfels criteria. From the point of view of 'direct experience', these properties of physical systems give us the laws of *Prägnanz* and of the transposable properties of *Gestalten.*

Now these properties apply to all physical systems characterized by the fact that the distribution of forces depends on the final distribution or end state. Since they apply to all systems in a perfectly general way, they are no more than ontological analogies. The danger of ontological analogies is that they easily come to be regarded as causal analogies, and when this happens, the stage of *description* in science is never transcended.[1]

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[1] In his review of Koffka's book Prof. Boring makes the same point even more strongly and develops it at greater length. *Psychol. Bull.* (1936), XXXIII, 59.

That the Gestalt theoretical analysis of the properties of the psychophysical field yields chiefly ontological analogies is, I think, confirmed when we look at the details of the laws of *Prägnanz* as set forth by Koffka. They do not seem to advance far beyond the descriptive stage.

"Psychological organization will always be as 'good' as the prevailing conditions allow" (p. 110). "Organization depends on the resulting form" (p. 137, in the discussion of the Benary effect). "Of several geometrically possible organizations that one will actually occur which possesses the best, the most stable shape" (p. 138). We see that these formulations are based on the underlying physiological theory. Now the Benary effect shows that the perceived brightness of a small field is not a linear function of the surrounding intensities averaged over the whole field (which is what the older sensationistic induction theories demand, cf. Plateau's and Talbot's laws) but depends (*a*) on the—often discontinuous—gradient of intensities in the neighbourhood of the small field; (*b*) on the spatial relations of the perceived fields. The same generalizations can be made about the Mach effect on a rotating colour wheel. There, too, a lighter ring appears where the purely summative induction theory would demand a dark ring. But formulated in this way, we can take the above effects and subject them to more detailed experimental analysis, in each aspect, the physical and the perceptual.

If we look at the special cases of the law of *Prägnanz* we see the same descriptive process: "a strongly *unified* part of the field will look as *uniform* as possible, *i.e.* as much as the prevailing conditions allow" (p. 135). The factors of 'closure', of 'good shape', of 'good continuation' and so on, will not allow of prediction in specific cases, since they are descriptive translations into the terms used for the dynamic equilibria of physical systems. The most direct consequence of the law of *Prägnanz* is the law of symmetry (p. 195): in the figure ground phenomenon "the distribution will...be such that the resulting shapes are as simple as possible" (p. 195).

The above analysis is not meant to imply that *experimental* Gestalt psychology is defective. It represents an immensely stimulating and fruitful beginning, which, like so many of the great beginnings in the history of the experimental sciences, has been brought about by following up certain analogies. We need only read the chapters on perception or on the constancies to be plunged at once into an exciting world of fascinating experiments and observations. The labours of this school should now have laid the ghost of mechanical sensationism. Not the least valuable part of the book is perhaps the emphasis it once more gives to the need for qualitative, phenomenological, introspective data in psychology, a need that Behaviourism had tried to deny.

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[1] The distinction between causal and ontological analogies is treated at greater length in Oeser, 'Critical Notice: Gestalt Psychology and Gestalt theory', *Brit. J. Psychol.* (1930), XXI, 73.

Gestalt psychology thus links up with the phenomenological work of people like Katz on colour and Jaensch on types. Koffka says, for instance, that the Ego-environment relation is not a purely cognitive one, but is conative as well, since the Ego adapts its behaviour to the Environment, and that the term 'instinctive' is an observational term describing this fact. He goes on: "when we say: Women rely more on instinct than men, we mean just this, that in a woman's behaviour the Ego is less separated from the environment than in a man's, and that therefore the mutual interplay of forces which determines the behaviour on the one hand and the physiognomic characters on the other is stronger" (p. 361). Jaensch uses practically the same words when characterizing the Integrate type.

Jaensch and Koffka also have this in common, that both tend to make sweeping generalizations on slender evidence, and neither pays sufficient attention to the social factors or 'forces' that must be defined in order to make terms like instinct, sentiment, and personality intelligible. What 'women' do or think we do not know. We can only know how women-in-Germany or women-in-Melanesia behave. The work of Mead and Benedict should have taught psychologists and psychoanalysts that at the level of integrated social behaviour it is completely fallacious to operate with abstractions like 'women' or 'the child'.

On the theoretical side Gestalt psychology links up with a strong tradition in British theoretical psychology, that of Stout: "We have...to determine whether the unity of the external world can be accounted for merely as due to acquired meaning, or whether, on the contrary, there is some apprehension of it, however rudimentary, from the outset....*If we are to secure ourselves against the fallacy of explanation in a circle, we must assume from the outset something answering, in however vague a form, to our developed consciousness of the world as a unity*—a system in which all parts are in various ways connected with each other. Further, from the outset this embryo awareness of unity has various aspects answering to the fundamentally diverse kinds of relations given in sense-perception. Among these...(are)...Spatial Unity, Temporal Unity, Causal Unity and the Unity of different Attributes as belonging to the same thing. Such forms of unity may be called Categories.... *Our position is that such categories belong to rudimentary perceptual consciousness as a condition of its further development*"[1] (*Manual of Psychology*, 3rd ed., pp. 436–8).

Stout's position has thus been verified by experiment.[2] The next step in the consolidation of Psychology is therefore also foreshadowed in Koffka's book. This is to start by restating the position italicized above, then proving the validity of this assumption from the experimental evidence collected by numerous workers in diverse fields, and building on this foundation a system of theoretical psychology.

It is interesting to compare the violent polemics which accompanied the early years of the Gestalt school with the urbane way in which Koffka evaluates the work of others and brings it into relation with his own theories. But it seems to me that this happy unification of psychology will not be easily achieved unless the Gestalt psychologists on the one hand jettison metaphysics and on the other give, and continue to develop, *operational definitions* of their concepts. On the side of the physical environment this process has begun, and numerous experiments demonstrate the steps by which perception can be interfered with, and the objective conditions on which it depends. But when we come to what Koffka calls 'the behavioural environment', we look in vain for definitions of behavioural objects, of meaning, of organization, of insight, of traces, to mention some of the many important concepts, and we find no clues as to how experience does in fact affect the organization of perception, of instincts, and of the higher forms of behaviour in general. The more complex behaviour is, the less possible is it to give *a priori* definitions. That is, perhaps, the reason why the chapters on the personality and on social organization are so unsatisfactory. Koffka himself realizes that they are, but does not attribute this to the same causes as I do. In what way Gestalt psychology still falls far short of the requirements of a systematic science of behaviour at more complex levels of integration may be seen when we read, for instance, Clark Hull's analysis of the experiments of Norman Maier on insight (*Psychol. Rev.* (1935), XLII, 219). Hull shows that Maier's invocation of Gestalten to 'explain' complex types of behaviour is tautologous since it simply reasserts, in different terminology, that there is a problem. Or, if we look at the work of B. F. Skinner, which has been appearing in the *J. gen. Psychol.* since 1932, we see how powerful a method experimental psychology becomes, when such terms as 'strength of a reflex', or 'general and specific choice' are defined with reference to measured responses in a controlled environment. In Koffka's book the technique of control experiments is hardly mentioned, and statistical methods might never have been developed. But when we demand experimental criteria, control experiments and statistics become essential; and I find it impossible to persuade myself that insistence on these is merely the fruit of an anti-metaphysical, positivistic intellectual climate.

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[1] Reviewer's italics.

[2] But the resemblance does not go much further than this. Koffka criticizes at length, for instance, Stout's conception of 'cumulative dispositions' in memory. Again, Stout distinguishes between sensible appearance and perceptual appearance. He would say that if a book lies on a table, the apparent continuity of the table underneath the book is the sensible appearance of the book-on-the-table. Koffka will have none of this, and maintains that in 'unbiased' direct experience we *see* the table (background) extending under the book without a break of continuity. Stout's distinction seems to me a step towards making comprehensible the influence of experience, an influence Koffka is at pains to ignore.

PUBLICATIONS RECENTLY RECEIVED

Principles of Animal Psychology. By N. R. F. MAIER and T. C. SCHNEIRLA. London: McGraw-Hill Publishing Co. 1935. Pp. xiii + 529. 24*s*. net.

A book of this kind, "designed to serve as a systematic text-book of animal behaviour for courses in psychology and biology", has been badly needed for a long time, and the present work should serve its purpose admirably. It treats the whole subject in a well-balanced way; and, in spite of the necessarily condensed nature of the description of so much experimental work, it is clearly written. There are many references to recent work, a number of excellent illustrations, and an extensive bibliography.

Part I, which occupies more than 250 pages, takes the classes of animal from Protozoa to birds, and develops from summaries of typical experiments an account of the behaviour capacities of each. A useful feature is a table for each class of animal, summarizing the receptor, conductor, and effector equipment of that class. Part II discusses "Natively determined behaviour", "Differential reaction to stimuli", and "Neural mechanisms in behaviour" in the higher animals. Part III is concerned with modification of behaviour in mammals, and ends with a discussion of reasoning.

The authors have attempted to give a general account of the present state of the study of animal behaviour rather than to put forward any theory of their own; and they have succeeded in producing a very valuable text-book.

The Psycho-Biology of Language. An Introduction to Dynamic Philology. By GEORGE KINGSLEY ZIPF. Boston: Houghton Mifflin Company. (London: George Routledge and Sons.) 1935. Pp. ix + 336. $3.50. 12*s*. 6*d*. net.

A very interesting account of the author's work upon the relation of the complexity of a linguistic element to the frequency of its occurrence. Considering a number of different elements, such as words, syllables, phonemes, etc., and stating his criterion of complexity in each case (length, for instance in the word), he shows, in the case of a number of languages, that there is an equilibrium between complexity and frequency. If one of these quantities alters, the other will also, and in such a direction as will tend to the re-establishment of equilibrium. In the case of word length, equilibrium is defined by an inverse (not necessarily linear) relationship between the length of the word and its relative frequency. On the basis of such considerations as these, the author advocates a dynamic and objective science of language which should be of considerable interest to psychologists.

An Ordinary Life. By KAREL CAPEK. London: George Allen and Unwin, Ltd. 1936. Pp. 245. 7*s*. 6*d*. net.

Probably most psychologists prefer their leisure reading to take them far from the world in which one potters about among emotions, motives and inner experiences. Then they had better read the book during their working day, for assuredly they will find this beautiful and subtle story of far more significance than many a portentous treatise. It is in the form of an autobiography, in which an old station-master, near to death, writes the story of his ordinary life. An 'ordinary life', to outer seeming, it may have been; but lived by no ordinary mind. That is perhaps the minor difficulty which will bother most of us. How should this ordinary railwayman see so clearly the little adventures of his childhood shaping all his mature experience and behaviour? How should he, even when his last journey is near, write with such direct clarity, such insight, such charm as are here displayed, about the destinies that shape a man? So he must protest his 'ordinariness' again and again. He must repeat again and again, as if by repetition he can make things look usual. But this is a small matter after all. Many a man, master perhaps in his own station in life, will gain insight and stability as he reads this book, and learn much about the influences which have shaped his conduct. He may gain, also, a greater understanding of his fellow-men.

Occupational Abilities: A Study of Unemployed Men. By N. W. MORTON. Oxford University Press. 1935. Pp. xxiii+279. 10*s.* 6*d.* net.

This very competent study merits the serious attention of all people who are interested in one of the most pressing of modern social problems, whether they are psychologists or not. Naturally, however, the psychologist with a fairly good knowledge of current methods of testing capacities and character will gain most from its perusal. The work described has been carried out upon unemployed groups in and around Montreal, and all the results are presented with the most admirable caution. A very large number of tests of one sort and another was used, and statistical analysis is given wherever this is possible. The author clearly and fairly points out many of the main difficulties, whether of technique in test application, or of obtaining reliable samples for test purposes, which he met, and makes valuable suggestions for dealing with them. Nothing very sensational is achieved, or could be expected. On the whole, as regards capacities it appears that these particular unemployed groups did not differ much from the normal, and as regards the study of the conditions which determine the use of these capacities evidently further work, with better methods, is greatly needed. As Dr Morton says: "In the advanced study of occupational specifications and occupational classification, one of the most urgent necessities is the development of suitable criteria of jób satisfaction and competency. It is imperative that further studies should deal aggressively with this important factor, and that they should in addition pay considerable attention to the problem of the relation of personal capacity to training and to finished skill." With this conclusion everybody who has attempted any first-hand observation, or made any serious study of the research done so far, is likely to be in complete agreement. Both Dr Morton and Prof. Kellogg, under whose general direction this work was done, are to be congratulated upon a significant contribution to the solution of some very difficult and important problems.

Youth and Leisure: A Survey of Girls' Organisations in England and Wales. By MADELINE ROOFF. London: National Council of Girls' Clubs. 1935. Pp. ix+264. 2*s.* 6*d.*

This survey was prepared at the request of the Trustees of the Carnegie United Kingdom Trust under the auspices of the National Council of Girls' Clubs. Much of the information recorded was obtained by means of a questionnaire, which was surprisingly well tackled by a large number of people. Selected areas were observed directly, either by Miss Rooff or by some assistant. As the whole work had to be completed in a single year it is clear that the direct investigation had to be less-thorough than could be desired. But the result justifies the pains that were taken. It is now possible to get a fair picture of the activities of girls' clubs in this country, and to some extent to estimate not only what they try to do, but what they actually achieve. The Report contains very valuable material from which the social psychologist who wants to get beyond mere theorizing can begin. It is not written as a contribution to psychology in any sense, but it does in fact contain matter which the psychologist should know and which he could most usefully employ. It is greatly to be hoped that Miss Rooff will be given the opportunity to pursue her researches still further.

Psychology and Modern Problems. Edited by J. A. Hadfield. London: University of London Press. 1935. Pp. vi+241. 5*s.* net.

The problems discussed are: "National Character and National Sentiment", by Prof. Morris Ginsberg; "Psychology and Racial Differences", by Prof. C. G. Seligman; "Liberty, Authority, Democracy", by Ramsay Muir; "Educational Ideals and the Destinies of Peoples", by Dr Crichton Miller; "Psychological Aspects of Marriage and the Family", by Prof. J. C. Flugel; "The Artist in Modern Civilization", by Dr Emanuel Miller; and "Psychology and the Future of Religion", by the Very Rev. the Dean of St Paul's. The whole is prefaced by an excellent Introduction by Dr J. A. Hadfield, who endeavours with success to show that a thread of connection holds together the discussions of these various topics. The separate contributions were delivered as a course of lectures at the Institute of Medical Psychology. Every lecturer obviously took his task seriously, and the result is a challenging book on a high level, and full of material for thought and investigation. Within the limits of a brief notice it would be impossible to do justice to all the contributors, and the best that can be said is to assure the prospective reader that here is a book in which he will obtain much sound information and be introduced in a fascinating manner to problems well worthy of his serious consideration.

The Diagnosis and Treatment of Behavior-Problem Children. By H. J. Baker and Virginia Traphagen. New York: The Macmillan Company. 1935. Pp. xiv+393. 10*s.* 6*d.* net.

Nobody now doubts the importance of the early diagnosis and treatment of difficult children. Equally nobody knows very much about how to do it. This book should help. It gives an account of work based mainly on the "Detroit Behavior Scale". Sixty-six items which are held to be of diagnostic importance are placed upon a list. Then these items are considered in detail and methods of acquiring information about them are described together with a fair amount of illustration. General discussions of the significance of the whole programme are added, and it is freely admitted that reasonably promising work has been only just begun. The book, especially in its more concrete parts, undoubtedly has practical value. Since, as the authors themselves state, such diagnosis as they are interested in can never be a merely routine application of a standard technique, it looks as if the present work, and other investigations of like order, need to be supplemented by a direct study of the characteristics which make a good diagnostician.

Psychology and Practical Life. By Mary Collins and James Drever. London: University of London Press. 1936. Pp. viii+307. 5*s.* net.

This is an excellent book which is suitable for the general reader, the student of education and child development, and as an elementary text-book in applied psychology for the psychological student. It is pleasantly and clearly written. It might perhaps have been interesting to have devoted a little more space to some of the recent work on children, particularly that of Charlotte Bühler, and its applications to the treatment of children in the home and the nursery school; while the consideration of psycho-pathology, a highly specialized subject unfit for application by the layman, might have been omitted. But apart from this, the book supplies most adequately the need for a general text-book on applied psychology written from the English rather than from the American point of view. Its very moderate price will also make it welcome to the student.

The Ministry of Women. Report of the Archbishops' Commission. London: Church House. 1935. Pp. 90. 1*s.*

As is now well known the Commission were adverse to the admission of women to full priesthood in the Church of England. The main reasons have nothing to do with psychology, but section VI of the Report and an Appendix by Prof. Grensted deal with psychological and physiological considerations which evidently took up a good deal of the time of the Commission. The two main things revealed are: a great conflict of individual views on the matter—a fact well known before the Commission began to work—and a tendency to rely upon current theories of the psychology of sex in order to explain the violently adverse views expressed in many quarters. No doubt, as is said, "powerful subconscious motives" may have much to do with the position both of those who support and of those who oppose the proposals which were being considered. These may or may not be based, as Prof. Grensted rather inclines to think, on "infantile conceptions" of the type of the Oedipus and castration complexes. But it seems a pity, if publicity was going to be given in this Report to such suggestions, that some more detailed consideration was not given also to possible direct social causation. It may be, of course, that the reluctance to admit women to full priesthood is fundamentally based upon unconscious 'ideas' of male dominance, and that these somehow belong to the natural history of psychological development in every individual. But it may also be that a vast amount is contributed directly from a social culture which is biased in this direction and which most people accept without criticism.

Melancholia in Everyday Practice. By E. L. HOPEWELL-ASH. London: John Bale Sons and Danielsson, Ltd. 1934. Pp. 136. 7*s.* 6*d.* net.

It is to the general practitioner that this book is directed. The author's hope is that he may aid "the proper recognition of melancholia in its less evident types in general practice". He gives a straightforward description of the clinical characteristics of various melancholic states, with some case histories, a discussion of methods of treatment, and a not very thoroughgoing consideration of etiology. The book is sane, well balanced and clear, so far as it goes. Though it is rather conservative in its general point of view, it gives a fair recognition to the importance of contributions to an understanding of the types of case dealt with which are based on psychological facts. It is not sympathetic towards psycho-analysis.

The Physical and Mental Growth of Prematurely Born Children. By JULIAN H. HESS, GEORGE J. MOHR and PHYLLIS F. BARTELME. University of Chicago Press and Cambridge University Press. Pp. xxiii + 449. 22*s.* 6*d.* net.

Part I consists of a "Clinical Study", by Julian H. Hess; Part II of "Developmental Studies of Prematurely Born Children", by Drs Mohr and Bartelme; Part III of "Special Studies Carried out in the Premature Infant Station at Chicago", and is written by various doctors attached to the Institution. The whole represents a very fine piece of co-operative research, and is far ahead of any other work of the kind that has yet been carried out. Plenty of indications of necessary further research are given, but as it is the book must take at once a position of great authority.

On the Bringing up of Children. Edited by JOHN RICKMAN. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. xvi + 237. 6*s.* net.

Dr John Rickman has prepared this book from a lecture course given in London in 1935. The six lectures which are printed deal with: "Planning for Stability", by Ella Freeman Sharpe; "Weaning", by Melanie Klein; "The Uses of Sensuality", by

Dr M. P. Middlemore; "Questions and Answers", by Nina Searl; "Habit", by Susan Isaacs; and "The Nursery as a Community", by Susan Isaacs. Every lecture is authoritative within the school of thought represented and is written in that sense. From the point of view of the ordinary parent, whose children grow up without any undue strain—if one may accept the evidence of their behaviour—it is decidedly difficult to evaluate this kind of book. Such a parent may either be tempted to dismiss much of what is said as imaginative rubbish, or he may get himself into a state of horrible worry, or he may be merely amused. But for the parent who is up against real difficulties in the upbringing of his children the position is somewhat different. Provided he has himself enough sanity and balance not to be over-worried by what is said here, the book will do him good. All the same, it ought to be read critically, or it may help to produce just that state of worry and apprehensiveness that it is intended to help to remove.

Speech in Childhood: its Development and Disorders. By GEORGE SETH and DOUGLAS GUTHRIE. Oxford University Press; London: Humphrey Milford. 1935. Pp. x+224. 10*s.* 6*d.* net.

One of the good features about this extremely good book is the emphasis which it places upon the social importance of speech, and of disorders of speech. The parallel disorders of vision are fully recognized, and nowadays are adequately, or almost adequately, dealt with. But troubles of speech and especially allied troubles of hearing produce social disabilities which may be followed by an even more disastrous sequence of results. If this book, with its careful critical analyses, its record of original work, its judicious and unexaggerated statements, calls attention to the urgent need for a more public recognition of the timely treatment of speech disorders, it will have accomplished a very fine piece of work.

Apart from this the volume will greatly interest any student of psychology, anybody who is interested in the education of the young, and most medical practitioners. Excellent accounts are given of the mechanism of speech and of the present position with regard to the cerebral processes involved in its production and understanding. Speech sounds are analysed clearly, the normal development of speech recounted, and the relation of hearing and speech considered judicially and with close regard to facts. Disorders of articulation, nasal speech and stuttering are all discussed, and valuable suggestions made for the public treatment of such disorders. There is a chapter on "The Singing Voice in Childhood". Apart from the special section which is devoted to a study of "The Psychology of Speech", psychological considerations are to the front throughout. References to the main authorities are given fully. It is a good book and deserves success.

The Psychology of Christian Personality. By ERNEST M. LIGON. New York: The Macmillan Company. 1935. Pp. xi+393. 12*s.* 6*d.* net.

Here are eleven 'lay sermons' written on the general text of the Sermon on the Mount. The teaching of Jesus is interpreted in terms of some of the doctrines of recent psychology. The book is very well expressed and very honest. It does not pretend to any particular originality, and not a little of the psychology is rather dragged in, and applied in a somewhat uncritical manner.

Passivity and Rationalisation. By NATHAN FIALKO. New York: John Lowell Pratt. 1935. Pp. 214. $2.50.

Mr Fialko has written a queer mixture of current social history, metaphysics, psychology and economics. From his welter of words it is far from easy to find exactly what he wishes to maintain. Apparently, however, the ***Man in the Street***, by some sort of exercise of his active and passive capacities, is attaining a kind of rational outlook by means of which he will be able to "renovate the Truth".

Behavior, Knowledge, Fact. By ARTHUR F. BENTLEY. Indiana: The Principia Press; London: Williams and Norgate, Ltd. 1936. Pp. xii+391. 15*s.* net.

This tremendously long book begins by selecting and rather abstrusely criticizing a number of representative views in modern psychology. Mr Bentley prefers the position adopted by Prof. Kantor, and called "organismic". He applies this to a study of facts, both objective and social, in order to show how science may "grow immensely in firmness of research and interpretation". It is a difficult book.

Objective Evolution. By C. PLEYDELL-BOUVERIE. London: Williams and Norgate, Ltd. 1936. Pp. 233. 7*s.* 6*d.* net.

It is extremely difficult to give a fair description of this book in a few words. The author thinks that man is on the verge of immense and far-reaching changes. As in the past sensorial reactions have been somehow combined and integrated to give rise to conscious processes in touch with an external world, so now conscious processes themselves are pressing towards the development of new and more stably founded psychological values. In order that these should be conserved and developed, new educational, social and, above all, economic policies must be developed. The economic policy advocated is the formation of a new "Consumer Currency" system. The great gains which the author anticipates from this are stated, but, though the matter goes far beyond the scope of the mere psychologist, it hardly seems as if sufficient indication is given of how the system itself could possibly be instituted.

Psychology: An Elementary Text. By L. B. HOISINGTON. New York: The Macmillan Company. 1935. Pp. xxi+557. 10*s.* 6*d.* net.

Prof. Hoisington says that the claim to consideration of his book "rests on the attempt which it makes to further the trends of thought inaugurated by such writers as Bentley, Dunlap, Koffka, Köhler, Kruger, Langfeld, McDougall, Washburn, Warren, Woodworth and others". This, no doubt, is only a way of saying that the text-book is up-to-date and comprehensive. As a matter of fact, with the exception of Woodworth, not much detailed use is made of the writings of the authorities cited in this list, and many other contributors receive much more extended recognition. The book covers a vast range, and with a basis of lectures will be found as useful as other text-books of a similar level. But it is rather difficult to believe that a beginner in psychology who is working by himself and without much access to other books could find this one altogether satisfactory. It may, however, well give the student with a library at his back quite a good start, and he will find plenty of reasonably well chosen references to aid him in pursuing further reading.

The Teacher in Training. By J. C. HILL. London: George Allen and Unwin, Ltd. 1935. Pp. 160. 5*s.* net.

On the whole this book bears the appearance of a collection of lectures or brief essays rather than that of a considered treatise. It is, of course, none the worse for that, so long as the fact is realized. The discussions are lively, interesting and well illustrated, just dogmatic enough to challenge criticism, but in no sense overbearing. Only Part I deals with "The Teacher in Training", and, apart, perhaps, from the first chapter on "Class Management", the title seems a little arbitrary. The other subjects considered are: "Mind and Body" (in the main an elementary statement of the functions of the central nervous system), "The Dream Mind", "The Child Mind", "Creative Work", "Education for What?" and a "General Conclusion". The second part is devoted to "Suggestions on the Teaching of Various Subjects", and is definitely practical in aim.

Appendices deal with "Voice Production", "Left-handedness", and an attractive collection of quotations on "The Creative Mind". The author has been considerably influenced by current views about the unconscious mind. In so far as books can help the prospective or the practising teacher, this small volume should be of value.

The Teacher's Guide to Intelligence and other Psychological Testing. By E. P. ALLEN HUNT and PERCIVAL SMITH. London: Evans Brothers, Ltd. 1935. Pp. 94. 1*s.* 6*d.* net.

This practical manual on the testing of intelligence, of special aptitudes and of temperament ought to be of much service. Some theoretical questions connected with the techniques presented are briefly discussed; tests themselves are well illustrated, and there are good instructions concerning the application of tests.

Behavior Development in Infants: A Survey of the Literature on Prenatal and Postnatal Activity, 1920–1934. By EVELYN DEWEY. Published for the Josiah Macy Jnr. Foundation by Columbia University Press; London: Humphrey Milford. 1935. Pp. viii+321. 17*s.* 6*d.* net.

Anyone who has endeavoured to keep abreast of the enormous amount of work which has been done in the last few years on the activity of very young children will be grateful for this clear and also, in some respects, critical summary. After a rather brief section on growth, the author deals in turn with pre-natal, neo-natal and infantile behaviour. The book is in no sense merely a list of authorities, though the full bibliography is given at the end, but is a running commentary on the main and important contributions. The difficulties in the way of getting an adequate picture of growth development, and also the modern trends of interpretation, are cleverly brought out. For student and teacher alike this is a book of value.

The Dream in Primitive Cultures. By J. S. LINCOLN. London: The Cresset Press. 1936. Pp. xiii+359. 18*s.* net.

In his introduction to this book Prof. G. C. Seligman stresses the fact that it is not "an attempt to provide additional proof of the validity of any particular school of psychological teaching, but is an honest essay in the application of psychological knowledge to some of the fundamental problems of cultural anthropology". This is perfectly true, although the author is naturally enough predisposed in the direction of Freud and his teaching. But much of the value of the book lies in its wealth of illustrative material, which is much greater than anything else so far published on this kind of topic in English. The author has ranged widely for his material, though he was started on his quest by his own researches among the Navaho Indians. He always makes a serious attempt to relate the dream to its cultural setting, and in the result has not only produced a very useful source book, but has formulated problems and lines of solution which should stimulate much further research.

The Delinquent Boy and the Correctional School. By NORMAN FENTON. Claremont, California: Claremont Colleges Guidance Center. 1935. Pp. 182. $1.50.

This monograph gives an account of the methods adopted in the treatment of delinquent boys at the Whittier State School of California. It will be found useful by all in this country who have the institutional care of similar cases. Prof. L. M. Terman contributes an appreciative preface.

A Text-Book of Psychiatry. By D. K. HENDERSON and R. D. GILLESPIE. Oxford Medical Publications. London: Humphrey Milford. 1936. Pp. xii + 606.

It is a pleasant duty to record the appearance of the fourth edition of this text-book. The volume has been revised and brought up to date, and will certainly continue its career of thoroughly deserved success.

The Mother's Encyclopaedia. Edited by LEN CHALONER. London: George Allen and Unwin, Ltd. 1936. Pp. 692. 8*s.* 6*d.* net.

So far as books can help, the contemporary parent should have no trouble in bringing up his child in the way he should go. Here is a new one, very inexpensive and as non-technical as possible. It is written by a large number of American authorities, and deals with a large number of possible and, one would think, impossible questions. On the whole it is a good production, and not at all alarmist in tone, though sometimes a bit lacking in humour. It may be found helpful by many mothers.

Dreams in Old Norse Literature and their Affinities in Folklore. By G. D. KELCHNER. Cambridge University Press. 1935. Pp. 154. 10*s.* 6*d.* net.

Over five hundred dream references are to be found in old Norse literature. Here a considerable number are selected and analysed and the texts and translations printed in an appendix. It is a little doubtful how far they represent a literary convention and how far they reproduce actual human experiences; but the author cannot be far wrong in maintaining that most of them at any rate must have a basis of the latter. Certain of their leading elements are brought out in the analysis: the coming in of Christian influences, the preponderance of adversity dreams, the character of the symbolism appearing in them. Miss Kelchner very wisely abstains from much interpretation along psychological lines, but it is a little disappointing that she does not make a more determined attempt to relate them to their general cultural background so as to bring out more clearly their probable social significance.

Friendship-Love in Adolescence. By N. M. IOVETZ-TERESCHENKO. London: George Allen and Unwin, Ltd. 1936. Pp. xiii + 367. 16*s.* net.

Here is a study, very largely based upon copious quotations from detailed diaries kept by two boys and a girl during adolescence, and supplemented by other original material as well. The author, first defining the term sexual very carefully, narrowly and sensibly, shows clearly enough that these particular cases of love-friendship are very far from being merely a sexual matter. The relationships involved may, indeed, perform functions in a developing emotional and intellectual life of the utmost value, and of genuine service. Whether the book need have been quite as long as it is in order to demonstrate this may be doubted. But if it corrects a number of one-sided views among psychologists and others it will have been well worth while.

God or Man, a Study of the Value of God to Man. By JAMES H. LEUBA. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1934. Pp. xii + 332. 10*s.* 6*d.* net.

For the general reader Leuba presents some psychological aspects of the experience and expression of religion, which he considers as the relation of man with personal gods. He omits any discussion of the function of religion as socializing the isolated individual, and adds nothing new in theory or by experiment to the understanding of the psychology of religion as an experience. While the argument is given with clarity, it is often supported by dogmatic statement of debatable points, on the basis of which it is not safe to generalize.

La Formation des Habitudes. Par P. Guillaume. Paris: Félix Alcan. 1936. Pp. 206. 15 frs.

Though there is nothing vastly original in this work, it gives a very useful and clear summary of a great amount of experimental investigation of the formation of habits. In its conclusions it leans strongly towards dynamic concepts and away from an associationist view, which, however, is recognized as possessing value in certain respects. The general conclusions are applied to pedagogical problems.

L'Ironie. Par V. Jankélévitch. Paris: Félix Alcan. 1936. Pp. 151. 10 frs.

Here is a book written with great skill and delicacy. It carries learning lightly, and can be read with intellectual delight. It is all on a very general plane, but is full of acute remarks and felicitous illustrations. The three chapters are entitled: "L'ironie est la conscience"; "Les visages de l'ironie. De la Simulation"; and "Des Pièges de l'ironie". Everything is beautifully precise, and in its own style the essay can be awarded a high place.

La Psychotechnique: Introduction à une technique du facteur humain dans le travail. Par P. Sollier et J. Drabs. Comité Central Industriel de Belgique. Paris: Félix Alcan. 1935. Pp. xviii+189. 17.50 frs.

The book gives a clear and comprehensive account of the methods developed, mainly under the inspiration of Dr Paul Sollier, for the systematic examination of vocational capacities. Tests, many of them exceedingly ingenious, and questionnaires are widely used, and attempts are made to cope with problems of personality which at present are hardly amenable to test methods of a routine character. The book is well illustrated. It is interesting to see what a relatively small part is played by statistical method in this development of industrial psychology, and how much greater emphasis is laid upon qualitative examination. The volume is mainly produced by Dr José Drabs, who writes to begin with an excellent and appreciative account of the work of Sollier, whose unfortunate death must have been a severe blow to the work of industrial psychology in Belgium.

Recreation and Education. London, International Labour Office: P. S. King and Son. 1936. Pp. viii+151. 4*s.*

In this Report are collected the papers presented, on the topic indicated, to the International Conference on Workers' Spare Time held in Brussels in 1935. The main subjects dealt with are music, the cinema, broadcasting and public libraries. Accounts also are given of organized efforts to provide spare time educational and recreational facilities for working people in most of the European countries. All the discussions and reports are in general terms. They represent lots of hopes, aspirations and recommendations, not much in the way of reliable investigation. If they suggest problems for research and ways in which the necessary social observation and experiment can be carried out they will have served a useful purpose.

Premiers Principes d'une Théorie Générale des Émotions. Pa
LATOUR. Paris: Félix Alcan. 1935. Pp. 651. 50 frs.

This is a new and enlarged edition of a work first published in 1912.
has been considerably influenced by the later works of M. Pierre Janet, an
publications of M. Lévy-Bruhl. His general orientation is philosophical,
a mass of valuable and original observations and analyses embedded i
treatise. After discussing the Will and its attributes, M. Marius Latour
consider at length, and under various specific headings: instincts conn
conservation and growth; those involved in an effort to achieve freedo
immediate control of a pressing environment, whether social or indivi
siderations with regard to problems of morality and art and involving a stu
he calls *le réel et le vrai*; and finally he attacks the question of the cause an
the emotions. It will be obvious from his general scheme of treatmen
interests are largely concentrated upon the activity significance of emotional
and reaction.

Essai sur la Création Artistique. Par LIVIU RUSU. Paris: Féli
1935. Pp. 463. 50 frs.

The author indicates that the title of his book should have been: "A
artistic creation regarded as a revelation of the sense of existence: A cont
a dynamic Theory of Aesthetics." Thus he does not regard himself as deal
psychological problem merely. "Our essential conclusion is that the w
springs from an attitude in and through which is revealed, under some
vision of outer things, the deep sense of existence." Suppose, then, that on
the matter from the point of view of the artist himself. It appears that a v
must be regarded as a creative or constructive effort by which the artist,
all kinds of external distractions, endeavours to re-establish some lost eq
This general thesis is argued in an original and attractive manner, wit
illustrations, and with due regard to many other hypotheses. There is pe
great a tendency towards rather easy categorization, but undoubtedly Dr
produced an essay of a stimulating and valuable character.

Le Sens de l'Existence dans la Poésie Populaire Roumaine. P
RUSU. Paris: Félix Alcan. 1935. Pp. 123. 15 frs.

In this very interesting essay Dr Liviu Rusu applies the conclusions of
book on artistic creation to the popular poetry of Roumania, which has been
as "the most beautiful, the most profound and the most moving of all fo
He deals particularly with attitudes towards solitude and fate.

Writing and Writing Patterns. By MARION RICHARDSON. In tw
hinged cards and five books of Copies and a Teacher'
London: University of London Press. 1935. The complet
(Each Item can be purchased separately.)

"These cards and books are designed to give a child practice in the es
a simple running writing, which will serve him throughout school life a
foundation of a good adult hand." To the psychologist it is probable that
interesting part of Miss Richardson's scheme will be the use which she
'writing patterns'. They can be employed as pre-writing exercises and ca
tinued throughout the whole of the learning period as long as a child desires
of these patterns are reproduced in the teacher's book, not as copies, b
developing them the child is to be given as much freedom as he likes, but

illustrations. It is clear enough that they can and do achieve much beauty of form and pattern, and can play a great part in child development. The whole scheme seems to be an excellent one and should be widely followed.

Performance Tests of Intelligence: A Series of Non-Linguistic Tests for Deaf and Normal Children. By JAMES DREVER and MARY COLLINS. Edinburgh: Oliver and Boyd. 1936. Pp. 55. 5*s.* net.

The second edition of this useful book differs from the first in that it publishes new norms, based upon wider application, and differentiating between boys and girls; it proposes an alteration in the scoring of the cube construction test, and it includes instructions for giving the tests to normal children.

Intelligence and Scholastic Attainment. By H. T. PARKER. Australian Council for Educational Research. Melbourne University Press in association with Oxford University Press. 1932. Pp. 64. 3*s.* 6*d.* net.

An account is given of a research, based upon the application of familiar tests, in the State Schools of Tasmania.

Occupations in Victoria. By G. R. GILES and JOHN R. LYALL. Australian Council for Educational Research. Melbourne University Press in association with Oxford University Press. 1932. Pp. 77. 4*s.*

An Advanced Test of General Intelligence. By NORA M. HALES. Australian Council for Educational Research. Melbourne University Press in association with Oxford University Press. 1932. Pp. 64. 3*s.* 6*d.*

The first of these Reports represents an attempt to discover the normal absorptive capacity of occupations in Victoria. It is a model for similar surveys, the periodic application of which is very much needed in this country if vocational guidance is to be practically successful. It is hoped to follow up this investigation by a similar enquiry of jobs within each occupation selected.

The second Report presents a group test of advanced intelligence for which a fairly high reliability is claimed.

Sexual Regulations and Cultural Behaviour. By J. D. UNWIN. Oxford University Press; London: Humphrey Milford. 1935. Pp. 62. 2*s.* 6*d.* net.

Herein the author sketches briefly the essential features of his views on the topic indicated which have been published in a larger volume already reviewed in this *Journal.*

Circumstantial Evidence. By C. E. RATCLIFFE. London: John Bale Sons and Danielsson. 1934. Pp. 184. 2*s.* 6*d.* net.

'This is the story of a man who runs through the scale of nearly all possible public religious movements in England, and finally finds peace in Secular, Labour and Co-operative organizations. It is written partly as a biography and partly as a novel. It is a document of some social interest, but not of any very direct psychological significance.

The Wisdom of Ignorance. By T. A. O'KEEFE. South Africa Central News Agency, Ltd. London: Gordon and Gotch, Ltd. 1935. Pp. 180. 4*s.*

The book is written as a novel and is described as "a psychic love story".

The Glory of the Living Sun: A World-Wide Appeal to Replace Present Superstitious Creeds by Genuine Religion which must be True, Rational, Universal, Exalting. By A. C. E. R. London: Heath Cranton, Ltd. 1935. Pp. 291. 7*s.* 6*d.* net.

Psicopatologia Funcional. Por W. RADECKI y C. PAYSSE. Buenos Aires: Aniceto Lopez. 1935. Pp. 151.

Computing Diagrams for the Tetrachoric Correlation Coefficient. By LEONE CHESIRE, MILTON SAFFIR and L. L. THURSTONE. Chicago, Illinois: University of Chicago Bookstore. 1933.

A PROGRAMME OF DESIRABLE INVESTIGATIONS ON THE TOPIC OF VOCATIONAL GUIDANCE

I. *Introduction* (pp. 119–120).
II. *Fundamental problems* (pp. 120–121).
  A. *General methods* (p. 120).
  B. *Methods of the interview* (p. 120).
  C. *Temperament, character, personality* (p. 121).
III. *Social, individual, sex and age influences* (pp. 121–122).
  A. *Social* (p. 121).
  B. *Individual* (p. 122).
  C. *Sex and age* (p. 122).
IV. *General problems in relation to occupations and tests* (pp. 122–123).
  A. *Occupations* (pp. 122–123).
  B. *Tests* (p. 123).
V. *Factorial analysis and allied psychological problems* (pp. 124–125).
  A. *Group tests of intelligence* (p. 124).
  B. *Tests of mechanical ability* (p. 124).
  C. *Tests of manual ability* (p. 124).
  D. *Other factors* (p. 125).
References (p. 125).

I. INTRODUCTION

Perhaps there is no branch of applied psychology in which more activity is being shown at present than in that dealing with the varied problems of vocational guidance. At the same time it is not too much to say that many of the investigations which are being pursued show a lack of co-ordination and of planning, and it seems highly desirable that some statement should be made of the outstanding problems in this field, so that the attack upon them may be concerted as well as energetic. Moved by this consideration, several psychologists in this country who are interested directly or indirectly in vocational guidance have collaborated in producing the following programme. No attempt has been made to discuss the problems which are stated, or to indicate the methods which may be available for their solution. It is not implied that the problems should be taken up in the order in which they appear on the programme, or that the enumeration of problems is itself in any way complete. The programme itself is not the official pronouncement of any organized body, or of any particular group or school. It is put forward in its present form in the hope that it may perhaps help to lead towards a widely organized attack upon questions which are not only of intense practical importance, but also possess profound theoretical significance in relation to the development of psychological research. The suggestions that make up this programme have come from many sources. It is the desire of their authors that they may lead to further suggestions, to criticisms, and, perhaps particularly, to a consideration of the principles and methods which should direct and control organised effort in this field of psychological study.

II. FUNDAMENTAL PROBLEMS

A. *General methods*

Careful studies are needed (*a*) of the nature and tenability of the psychological assumptions which underlie the statistical investigation of the mental aptitudes and qualities that determine vocational guidance; (*b*) of the theoretical and practical considerations affecting the construction and employment of rating scales and the statistical use of their results; and (*c*) of the comparative results of the questionnaire and of other methods employed in work on temperamental qualities.

B. *Methods of the interview*

Knowledge is lacking as to what cues are used by different interviewers in assessing temperamental and character qualities. How far are such cues as vocal inflexion, facial expression, bodily movements, etc., neglected? Which cues are the most reliable and how are they employed for vocational guidance purposes? What are the chief errors in interviewing, and how far may they be eradicated by appropriate training? What are the best methods of interviewing? For example, are more reliable results obtained when two interviewers record their judgments on a given person independently than when they co-operate, the one concentrating on interrogation, the other on observing consequent reactions? Experimental investigation is also desirable into the effects, during interviewing, of incomplete or faint cues, the ability to detect them and to follow them up, and as to how such detection is actually effected.

Can the usual method of the interview be largely replaced by substituting the required performance of various specific acts and by using the manner of performance as defining different traits of temperament and character?

Systematic research is needed to determine how the success of the interview is determined by the interrelation of the personality of the interviewer and that of the person interviewed.

C. *Temperament, character, personality*

How different aptitudes, temperamental and character traits, interests, etc., are differentiated, interrelated and distributed; how, in combination, conflict, etc., they form a 'personality'; and what important standardized types of personality arising from such integration can be described—these also await systematic investigation.

Another fundamental problem which needs enquiry is—how far should vocational advice be based on the *absolute* values of the aptitudes and qualities possessed by the applicant or on their *interrelation*; that is on the 'mental pattern', such as is shown in a psychogram? Advice as to an academic or technical career may well depend not so much on the absolute level of intelligence as on its relation to the special aptitudes required. Personality may similarly depend more on the relation between, than on the predominance of, certain temperamental and character qualities.

It is not enough to determine the possession of aptitudes and qualities. Knowledge is needed as to the conditions under which they are employable at their maximum efficiency, whether at tests or in occupational life. How do incentives, wants or 'drives', *e.g.* interests, ambitions and ideals, influence capacity? The relations between 'drives' and temperaments, and particularly the retroactive effects (direct or indirect) of any 'drive' on the connections between the elements of reaction preceding the satisfaction or thwarting of that drive (1), require further study both in man and in other animals. Co-ordinated team work between animal and human experimental psychologists and the vocational adviser would be here the most promising line of approach.

III. SOCIAL, INDIVIDUAL, SEX AND AGE INFLUENCES

A. *Social*

Research is needed into the influence of social groups on the distribution, stability and exercise of mental aptitudes, interests, temperamental and character traits, ideals, etc. Do vocational opportunities (*e.g.* in a coal-mining district) affect the distribution of vocational capacities and interests? How are the latter affected by modern improvements in transport and communication? A few selected areas need study from these aspects.

More satisfactory group tests of intelligence are needed for children of different types. There is special need for the investigation of perceptual and other non-verbal tests to be used for backward children. This holds also for adults in manual occupations who have left school for some time.

B. *Individual*

A prolonged study of individual differences is essential in the development, in a large number of different individuals, of their scholastic and occupational aptitudes and attainments, of their interests and qualities of temperament and character, and of the causes of individual differences in such development. For this and for other purposes, satisfactory cumulative record forms are badly needed. The ascertainment of what determinants of individual behaviour are sufficiently permanent for their use in vocational guidance is fundamental for further progress in the latter.

C. *Sex and age*

It has been asserted ((2), p. 64) that at the age of 14 performance tests of intelligence afford a poor measurement of g because they are so much influenced by specific factors—more in boys than in girls in whom, it has been suggested, the special abilities concerned develop later. It has also been stated ((3), p. 44) that a supposed specific factor for the appreciation of form relations appears later in girls than in boys. Both these statements need confirmation.

The value of performance tests for children needs further examination. Apparently a boy or girl may have a mental age of 16 in one performance test and of 10 in another ((2), p. 66).

Much further work is needed on the predictive value of various tests. It has been found ((3), pp. 46, 47, 73, 76) that certain performance tests of intelligence are sufficiently consistent from year to year only after the age of 12. Is this true for other performance tests? It has also been found ((3), pp. 43, 45, 73), from using the Stenquist assembly test together with the National Institute of Industrial Psychology's test for the appreciation of form relations, that mechanical ability cannot be safely predicted before that age. Is this true for more trustworthy tests of mechanical ability (*e.g.* Cox's)? Finally, the conclusion ((3), pp. 45, 46, 73–76) that tests of manual dexterity cannot be considered reliable for early vocational guidance needs reviewing in the more recent light (4) of distinction between group and specific factors, and between complex and simple tests, of manual ability.

IV. GENERAL PROBLEMS IN RELATION TO OCCUPATIONS AND TESTS

A. *Occupations*

A classification of occupations is needed according to the abilities and the temperament qualities required. A complete description, for example, of the different occupations in each of fifty firms would be valuable.

A further study is needed of the level and limits of intelligence for occupations of different social classes. We know only roughly the average level, but it would be also useful to know the approximate minimum and maximum of the range in each occupation.

It is believed that the effects of maladjustment are more serious in sedentary occupations: this needs confirmation.

Better methods are required for the accurate assessment of occupational proficiency.

B. *Tests*

Tests of mathematical, scientific, artistic, and linguistic ability, of literary appreciation, and of verbal facility are needed, better suited for secondary and public schools.

Tests of imagery—not so much of its mere vividness (this has hitherto been too exclusively studied), but of its availability and plasticity—seem well worthy of construction.

It would be useful if standardised tests could be compiled for the possession of general knowledge and out-of-school information.

Prolonged and carefully planned investigation is required to answer the following questions: Are 'factorial' tests (*e.g.* of mechanical and manual abilities) only useful for *broad* vocational guidance? Must 'analogous' or 'analytic' tests (devised directly from the study of a particular occupation) be employed for more *specific* guidance? Or will it ultimately be possible to employ 'factorial' tests, variously combined, in order to diagnose with near certainty successful aptitudes for any specified occupation?

Research is needed into the respective values of, and the relations between, tests that involve selective responses and those that involve constructive responses.

Investigations should be conducted into the relative influence of (*a*) previous routine practice in certain tests and (*b*) of systematic training in the general principles underlying them, especially tests of intelligence and of mechanical ability. The effects of familiarity with the same type of any one test need to be distinguished from the effects of familiarity with other types of the same test.

It would be interesting to know the relation between two methods of scoring tests (*a*) counting only right (or partly right) answers, (*b*) penalizing also for wrong answers.

A study of the 'will to succeed' in group-given tests would be valuable.

V. FACTORIAL ANALYSIS AND ALLIED PSYCHOLOGICAL PROBLEMS

A. *Group tests of intelligence*

'Verbal' tests. The score at any of the usual group tests of 'verbal' intelligence has been said to depend on two factors—*g*, or 'general' intelligence and *v*, the 'verbal' factor. It is doubtful whether, psychologically, either factor is unitary. Their psychological analysis is therefore badly needed.

'Performance' tests. The score at certain of these tests has been said similarly to depend on two factors—*g* and *F*, the 'practical' factor. Again, psychological analysis of the 'practical' factor needs undertaking. In this connection also the relation, if any, of *F* to tests for the appreciation of form relations needs further study.

'Social' tests. Can similar group tests be devised for the assessment of 'social intelligence', *i.e.* of the ability to manipulate not symbols and abstract ideas, nor concrete objects, but living persons?

B. *Tests of mechanical ability*

A single 'mechanical' factor has been statistically established (5), attributed to the ability to apprehend and to employ mechanical principles in the use of machines, etc. In these tests levers, pivots, wheels, strings, etc., were involved indiscriminately. What would result from reconstructing the tests so that the uses of these different mechanical agents and the principles which they involve can be studied separately? What is the relation of this single group factor (or its psychological components) to tests for the appreciation of form relations, and to the 'practical' factor (*F*) in performance tests of intelligence? Is it identical with *F*? In what proportions are these various factors and *g* required for success in different mechanical operations?

C. *Tests of manual ability*

A single 'manual' factor has likewise been established here (4), which appears to be more dominant, the more complex the test; whereas in the simpler tests of manual dexterity, and hence in simpler skills, various specific factors seem to predominate. What is the relation of this single group factor to the 'practical' factor in performance tests of intelligence, to coarse and fine manual work and to the specific factors in different manual occupations? Is this group factor measurable by the aestheto-kinetic tests (cf. (6)), and by such tests as the mouse-trap and tracing tests?[1]

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[1] Used, *e.g.*, at the Rheinisches Provinzialinstitut für Arbeits- und Berufsforschung, Düsseldorf.

Can adequate evidence be proferred in support of the conjecture that tests of manual ability involve two group factors—accuracy and speed (7)?

D. *Other factors*

Two factors, named X and Z, have recently been analysed (8) from a statistical treatment of specially scholastic attainments. X is believed to be a temperamental or character quality, determining interest and related therefore to persistence and reliability. Z appears to be especially prominent in 'number' tests, and in shop work and mathematics. Were it not for its appearance also (but to a very small extent) in English, it would be tempting to identify it with 'number ability'. Or is it another character or temperamental quality? Further research is obviously required here.

And so it is in regard to the analysis of character and temperamental qualities generally. What are the relations of temperamental traits to one another? What are the prime factors, if any, in temperamental traits and what are their relations to tests of perseveration and of the obsessional and anxiety varieties of psycho-neurosis (cf. (9,10,11,12))? Further research is needed to investigate the view that a general factor of emotionality exists, analogous to the general factor of intelligence (cf. (13)).

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POSTPONED UNTIL 1937.

ELEVENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

MADRID, *September* 6TH–12TH, 1936

FIRST COMMUNICATION

1. In accordance with the decision of the Congress at Copenhagen, the Eleventh International Congress of Psychology will be held in Madrid from September 6th to 12th, 1936.

2. The Government of the Republic, by decree dated January 25th, 1936, has officially declared and extended its patronage to the Eleventh International Congress of Psychology.

3. The Congress will include:

(*a*) *Symposia*, to be assigned to two or more psychologists; the manuscripts of these papers, which are to be not more than 10–12 pages each (pages of 400 words), must be deposited at the Secretariat of the Congress before May 1st, 1936, in order that they may be printed and circulated before the Congress. Each contributor will have 15 minutes to comment on his paper.

(*b*) *Commissions* ("round-table conferences"), for which groups of specialists on certain questions will be invited to study and discuss together a selected problem, presided over by a president or rapporteur, who will draw up a summary of the reports he receives, which summary will serve as a basis for the work of the Commission. Each Commission will consist of 8 to 12 members and a president.

No single report is to be more than 400 words in length.

The reports must be sent in before April 1st, to be printed and sent to the president, for him to prepare his general report.

(*c*) *Lectures* on current psychological problems; certain psychologists who have specially studied and investigated such questions or problems will be invited to give these lectures.

The duration of each lecture will be not more than three-quarters-of-an-hour. An abstract or synopsis must be sent in before May 1st, to be printed and included in the programme.

(*d*) *Communications*. In spite of the length of the programme, sessions will be reserved for individual communications, abstracts of which, not more than 10 lines (100 words) in length, must reach the Bureau of the Congress before June 1st, 1936.

The division into sections of the sessions reserved for these communications will be made later according to the number and titles of the communications received.

4. The Congress will be composed of *active members, associate members* and *corresponding members.*

Psychologists and authorities in related sciences will be admitted as *active members.* Active members have the right to present contributions and to take part in discussions. They will receive all the official publications of the Congress.

In addition, all those interested in the work of the Congress may be admitted as *associate members.* These associate members will not take part in the discussions of the Congress and will not receive the scientific publications, but they may be present at the sessions and may benefit from the advantages in travelling, excursions, visits, etc.

There will be accepted as *corresponding members* any scientific societies, Institutes, Libraries, etc., that wish to participate in the results of the Congress by making a covering subscription, and to receive the collected proceedings.

5. The official languages of the Congress will be: German, English, Spanish, French, and Italian.

6. The amounts of the subscriptions have been fixed as follows:

(*a*) Active members: 75 pesetas (including all the publications and collected proceedings); their ladies, 30 pesetas.

(*b*) Corresponding members: 100 pesetas (including the collected proceedings).

(*c*) Associate members: 40 pesetas.

7. The Bureau of the Congress is installed in the Institutio Nacional de Psicotecnia, Alberto Aguilera, 25, Madrid. All correspondence relating to the Congress should be sent to this address.

8. A demonstration of psychological apparatus, tests and other documents of psychological interest will be given during the Congress in some place near to the hall of session. A section for publications and psychological literature will also be organized. Notice of participation in this demonstration should be given on the back of the entrance form which will be sent with the second circular. Material must be dispatched to arrive in Madrid at least thirty days before the opening day of the Congress.

9. It has been arranged that the Compagnie Internationale de

Wagons Lits-Cook shall reserve rooms in the hotels at Madrid, and supply members with all information about their visit. It has also been arranged that the Compagnie shall draw up itineraries for travel both before and during the Congress, in order that members may be enabled during their stay to visit interesting monuments and places in Spain.

10. The Bureau National de Tourisme is also at the disposal of the members of the Congress at Madrid to give them all the travel information which might interest them.

11. During the Congress receptions will be organized; members and their ladies will also have the opportunity to visit the city of Madrid and its monuments, escorted by expert guides.

J. Germain, *Secretary General* E. Mira, *President*

Institutio Nacional de Psicotecnia,
Alberto Aguilera, 25, Madrid

Madrid, *February* 5th, 1936

VOLUME XXVII OCTOBER, 1936 PART 2

Book No. .... LIBRARY

ENVIRONMENTAL INFLUENCE ON MENTALITY

BY THE LATE SHEPHERD DAWSON

I. *Origin of investigation* (pp. 129–130).
II. *Plan of investigation* (p. 130).
III. *Tests employed* (pp. 130–132).
IV. *Results* (pp. 132–133).
V. *Inferences* (pp. 133–134).

I. ORIGIN OF INVESTIGATION

THIS inquiry was suggested by certain observations which were made in an investigation into the relationship between disease and intelligence. Over a thousand children in two different hospitals were tested individually by the Binet tests, and the results were collated with information regarding the homes of the children. Some of this is summarized in Table I:

Table I

	General Hospital for Children		Fever Hospital	
No. of rooms in house	No. of families	Mean I.R. of children	No. of families	Mean I.R. of children
1	215	89·2	24	89·4
2	496	90·5	129	94·3
3	113	94·1	57	96·4
4 or more	41	96·5	27	100·0
Average no. of rooms	2		2·45	
Correlation of I.R. and no. of rooms	0·146 ±0·022		0·191 ±0·042	

There seems to be a connexion, though not a very close one, between the intelligence ratios of the children and the size of the houses from which the children tested came; there is a tendency for the more intelligent children to live in more roomy houses. This does not prove that the size of the house affects the intellectual status of its occupiers, as might be contended by those who are anxious to maintain that improvement in the conditions of living will raise the intellectual level. Nor does it prove that superior intelligence is itself responsible for superior living accommodation.

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[1] A summary collated from provisional reports submitted to the Scottish Council for Research in Education of an investigation undertaken by the late Dr Dawson, a full report on which was in preparation at the time of his death. The expenses of the investigation were borne by the Research Council.

The correlation merely suggests causal relationship without indicating which is cause and which effect, a fact which has been overlooked in the interpretation of the results of many inquiries into the connexion between intellect and social status. The only satisfactory method of finding whether external conditions like those mentioned above have any effect on mentality is by using the well-known method of experimental science, that of varying one of the factors and finding whether this produces a concomitant change in the other.

II. PLAN OF INVESTIGATION

An opportunity of doing this presented itself in the change of environment brought about by the institution of slum-clearance schemes, and led to this inquiry. The aim, then, of this investigation was to find whether the mentality of children was affected by a change in housing, or, more specifically, to find whether transfer from a slum house to one in a rebuilding scheme had any measurable effect on the minds of children, in particular, on their educational progress, their general behaviour, temperament, and what is commonly called their intelligence. The plan was to test them as thoroughly as possible about the time of the transfer and one, two or more years later, and to compare the results of the two examinations. At the same time another group of children who were living in slum areas and were not to be removed to a slum-clearance scheme was to be used as a comparison group: they were to be given the same tests as the others at similar intervals of time.

It was not expected that any change that could be produced by these alterations in external conditions would be great, especially in intelligence, which is commonly believed to be an innate character that is comparatively constant through life. Hence it was decided to make the examination as exact as was possible in the time at our disposal; intelligence tests were applied, educational progress was estimated by means of reading and arithmetic, and estimates of temperament were made at the time of each examination. Each child was examined individually because tests given individually have been found to be more reliable than group tests.

III. TESTS EMPLOYED

The Binet intelligence tests as revised and extended by Terman were used. The scholastic tests had to be restricted to reading and arithmetic, partly on account of the time consumed in giving the tests, and partly because these are the subjects to which most importance is attached in the primary schools. Young children of 5–8 years of age were selected for the experiment, as it was thought that they might react more quickly to environmental change than older children, and their I.Q.'s could be determined with greater certainty. Hence the scholastic tests were of the simplest kind; they were tests 1 and 9 in Burt's "Mental and Scholastic Tests". These were selected because they had already been used extensively and had been fairly well standardized.

The reading test is called by Burt a reading (accuracy) or graded vocabulary test (Burt's Test 1). Strictly speaking, it is merely a test of word recognition, which is only one of the factors involved in reading, the other and much more important being the ability to grasp the meaning of what is read. There was some hesitation about using this test, for it implies a conception of the essentials of reading which seems to be quite incorrect, the view that it is primarily the recognition of individual words; this conception is indicated in the terminology of teachers who distinguish between 'reading' and what they call 'intelligence'; the former is word recognition, the latter comprehension of meaning, which, after all, is the essence of reading. There are other and better tests, for example, Burt's reading (comprehension) test, where a child reads a sentence containing some instructions, such as, "Take a pin from the box on the table", and then proceeds to carry out the instruction. In the end these comprehension tests were rejected because they imply a view of reading which is not, or at the time was not, universally held by teachers, if one can judge from their practice in the schools. In the minds of the children themselves—whatever may be the pious hopes of their teachers and inspectors—to read is simply to recognize correctly each word on the printed page and pronounce it correctly; in other words, reading is to them reading aloud without making any errors in word recognition. As the aim of our tests was to find what progress the children were making in their studies, it would have been unfair to have tested them by methods which demanded an attitude of mind with which they were not familiar; as reading was for most of them word recognition, it was only fair that *this* should be tested, and not comprehension. There is another reason for selecting this test, namely, that in the absence of definite training in reading for meaning, a comprehension test becomes an intelligence test, and it seems preferable to test intelligence explicitly by the recognized tests.

The arithmetic test was a test of what is commonly called mental arithmetic, that is, paper and pencil were not used by the children in finding the answers, and it was given orally (Burt's Test 8). This is far from being a complete test of arithmetical ability; the latter would include also written tests of ability to do the simple operations of adding, subtracting, multiplying and dividing (as they are done in school), as well as what are commonly called 'problems'. It was not possible to find time enough to make such a thorough examination, and it was felt that the oral examination would be the best compromise. The children were already familiar with oral arithmetic, so that they did not suffer as they would have done had they been given comprehension tests in reading. Further, oral work had the advantage of eliminating all difficulties due to inability to read well, and of testing both speed and the ability to think arithmetically.

IV: RESULTS

It had been intended to allow two or three years to elapse between the two testings, but so many of the children changed their school, often on account of evictions for non-payment of rent, that the interval had to be shortened to between twelve and eighteen months; even then about 20 per cent. of those who were given the first tests could not be traced.

The total number of children tested twice, and their ages at the time of the first test, are given in Table II.

Table II. *Showing the ages of the children at the time of their first test*

	Slum clearance			Control group		
Age years	No. of boys	No. of girls	Boys and girls	No. of boys	No. of girls	Boys and girls
4–4$\frac{11}{12}$	1	—	1	—	—	—
5–5$\frac{11}{12}$	18	25	43	6	6	12
6–6$\frac{11}{12}$	43	33	76	9	6	15
7–7$\frac{11}{12}$	36	36	72	6	9	15
8–8$\frac{11}{12}$	30	44	74	5	9	14
9–9$\frac{11}{12}$	10	13	23	—	—	—
Total	138	151	289	26	30	56

The results of the intelligence, arithmetic and reading tests are summarized in Tables III and IV. When the two tables are compared it will be noticed that, whereas half of the increases in the control group are positive and half negative, all the increases in the slum-clearance group are positive. Most of the latter are statistically significant and, although they are small, they give the impression that the change of environment has produced a slight, but very slight and just detectable improvement.

Table III. *Showing the intelligence and scholastic ratios of 'slum-clearance' children*

	Ratio	n	Means			P	r
			1st test	2nd test	Increase		
Boys and girls	I.R.	289	90·6 ± 0·47	92·1 ± 0·47	+ 1·5 ± 0·36	0·002	0·71
,,	A.R.	278	86·7 ± 0·36	87·5 ± 0·42	+ 0·8 ± 0·22	0·012	0·85
,,	R.R.	259	91·6 ± 0·71	98·0 ± 0·70	+ 6·4 ± 0·41	0·000	0·83
Boys	I.R.	138	91·7 ± 0·72	93·8 ± 0·72	+ 2·1 ± 0·41	0·000	0·83
,,	A.R.	131	89·1 ± 0·61	90·0 ± 0·65	+ 0·9 ± 0·37	0·050	0·83
,,	R.R.	126	91·7 ± 1·06	98·0 ± 1·06	+ 6·3 ± 0·54	0·000	0·87
Girls	I.R.	151	89·5 ± 0·62	90·6 ± 0·60	+ 1·1 ± 0·38	0·025	0·81
,,	A.R.	147	84·6 ± 0·38	85·2 ± 0·51	+ 0·6 ± 0·31	0·096	0·79
,,	R.R.	133	91·5 ± 0·99	98·0 ± 0·92	+ 6·5 ± 0·57	0·000	0·83

n = number of children tested twice.

$$\text{I.R.} = \text{intelligence ratio} = \frac{\text{Intelligence age}}{\text{Chronological age}}.$$

$$\text{A.R.} = \text{arithmetic ratio} = \frac{\text{Arithmetical age}}{\text{Chronological age}}.$$

$$\text{R.R.} = \text{reading ratio} = \frac{\text{Reading age}}{\text{Chronological age}}.$$

The value which follows the sign ± is the 'probable error' of the value which precedes it.

P is the probability of getting the stated increase of the given sign by mere chance in samples of the given size from a homogeneous population.

r is the intensity of the correlation between the first and second tests.

Table IV. *Showing results of tests on the control group which consisted of children who are still living in a slum area*

	Ratio	n	Means			P	r
			1st test	2nd test	Increase		
Boys and girls	I.R.	56	91·1 ± 1·14	91·7 ± 0·90	+ 0·6 ± 0·69	0·28	0·80
,,	A.R.	39	88·4 ± 0·91	86·2 ± 0·79	− 2·2 ± 0·70	0·02	0·67
,,	R.R.	39	102·3 ± 1·58	103·6 ± 1·66	+ 1·3 ± 1·18	0·23	0·73
Boys	I.R.	26	88·0 ± 1·51	90·1 ± 1·18	+ 2·1 ± 0·85	0·05	0·83
,,	A.R.	19	89·5 ± 1·31	86·1 ± 1·16	− 3·4 ± 1·03	0·01	0·66
,,	R.R.	19	100·8 ± 2·40	105·8 ± 2·50	+ 5·0 ± 1·86	0·04	0·71
Girls	I.R.	30	93·8 ± 1·60	93·1 ± 1·32	− 0·7 ± 1·02	0·31	0·77
,,	A.R.	20	87·3 ± 1·23	86·3 ± 1·08	+ 1·0 ± 0·89	0·23	0·70
,,	R.R.	20	103·6 ± 2·06	101·4 ± 2·16	− 2·2 ± 1·284	0·13	0·82

Headings are as in Table III.

V. INFERENCES

The tables show in both the 'transferred' and the 'control' groups a high correlation between the results of the first and second applications of the tests, both intelligence and scholastic. They show too that the average intelligence ratio, arithmetic ratio, and reading ratio (which measure attainment in relationship to chronological age) of both boys and girls in the 'transferred' group are very slightly higher at the second examination than at the first, whereas in the 'comparison' group about half of them are slightly higher and half slightly lower, which is what one would expect with no change. This means that while the 'comparison' group progressed at their normal rate, there is some ground for thinking that there was a slight, barely detectable acceleration in the 'transferred' group.

In estimating the significance of this finding it will be necessary to consider what acceleration might be expected in 12–18 months, and whether it is due to change in physical environment or to change in social environment.

The inquiry into temperamental traits revealed nothing conclusive, probably because the technique was defective: a satisfactory technique has still to be developed.

There were, however, certain side issues which yielded some interesting results. The City Health Visitors supplied reports on the homes, the medical cards provided information regarding the state of nutrition of the children, and the personal observation of the examiner provided some information regarding the care and attention that had been lavished on them. There appears to be a direct relationship between the condition of the child (his cared-for appearance) and the condition of his home, between his condition at the first examination and his condition at the second, between his state of nutrition at the first examination and his nutrition at the second, between the estimate of the examiner and that of the Health Visitor, and between the condition of the child and his intelligence ratio, but there is no relationship between his state of nutrition and the condition of his home; in other words, there appears to be a direct connexion between the intelligence of the child (and presumably of the parents) and his condition and that of his home, but none between these factors and his state of nutrition.

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THE PLACE OF AESTHETICS IN SOCIAL PSYCHOLOGY[1]

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I. INTRODUCTION AND DEFINITIONS

ALTHOUGH I had reason to believe that aesthetics has been somewhat neglected by the social psychologists, it was with some surprise that I found this topic actually omitted by the Encyclopaedia of the Social Sciences. Art has fared somewhat better. There is an article on the subject but it is primarily historical and contains little of psychological interest. The reason for this neglect is probably due to the attitude of the social psychologists themselves. For the most part, they either omit the subject altogether from their text-books or else give it scant space. The following are a few examples of the treatment the subject has been accorded. William McDougall in his famous *Social Psychology* notes in passing "that in many persons aesthetic appreciation of the beauty of a fine character and conduct may play a large part in the genesis of the ideal of conduct and of the sentiment of love for this ideal" (8). F. H. Allport in his *Institutional Behavior* names the aesthetic together with the scientific and religious attitudes as giving the full meaning to life, and describes the attitude briefly as "a sense of harmony, proportion, and balance, and that delicate precision of form and content which is the delight of the true artist" (1). Krueger & Reckless (7) devote a few paragraphs to the subject, in which they admit that aesthetic appreciation is not an instinct but is grounded on social experience. Kimball Young is somewhat more generous in his treatment, but it is interesting to note that he is more expansive on the topic of beauty in his *Sociology* (15) than in his *Social Psychology* (16), and F. C. Bartlett (2) dwells upon the social factors of art when he is writing more as a cultural anthropologist than as a social psychologist. Wundt devotes most of a large volume to the origin of art forms in his *Völkerpsychologie*, but there is little about the psychology of art as a social force. J. R. Kantor (6) has given a few pages to the thesis that aesthetic activities, although individualistic, are influenced by forms of culture and institutional stimuli.

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[1] Honorary Presidential Address before the New York Branch of the American Psychological Association, 11 April 1936.

It is not by mere chance that the modern social psychologist has included so little from this field in his writing. The reason, I believe, is that it has been difficult to obtain satisfactory quantitative results, and qualitative descriptions have been generally in terms of values which he considers more the material for philosophic than psychological thought. It is true that Santayana's poetic definition of beauty as "a vital harmony felt and fused into an image under the form of eternity" (10) leaves a scientific psychologist speechless. It is also true that values as such are the province of the philosopher. But Santayana's better known definition of beauty as objectified pleasure is an excellent starting point for a development of the psychological distinction between mere sensuous pleasure and that type of pleasure which is both aesthetic and social. The analysis of the distinction between these two forms of pleasure is decidedly the psychologist's business.

Of the many definitions which have been given of aesthetic pleasure, in order to differentiate it from other affective states, that one seems to me of special importance which emphasizes the social attitude of communication.

II. THE RELATION OF AESTHETIC TO PLAY ACTIVITY

In order to obtain the proper perspective of the social aspect of aesthetic response, it is instructive to compare it with the play activity to which many aestheticians attribute its origin. That play is genetically necessarily antecedent to art is to my mind doubtful. In many respects, however, the two activities are very similar. They both seem to be self-initiated and to be pursued for their own sake without any ulterior goal. That is to say, both activities are assumed to lack a practical aspect. However, 'practical' is a very tricky term, for both activities are supposed to be useful escape mechanisms. It will be remembered, for example, that Freud considered play to be an escape from a difficult conflict into a world of fantasy, and that the return to reality through the objectification of this world of our own making gives us art. Wundt also spoke of this objectification of fantasy, which will readily be seen to be similar to Santayana's idea of objectified pleasure. Now through this escape from conflict, which Kimball Young quite rightly considers more of an expansion of personality than strictly an escape, the individual is able to balance and integrate those activities which previously had been blocked. There is nothing more useful for the child than to be able to develop his activities in play. A world of make-believe, where the child can move things and people, which would otherwise be beyond his power, is, except for the extreme realist, a most desirable place in which to live for brief periods of his life. As to art, there are undoubtedly many persons who are so well adapted to the things about them that they never feel the impulse to turn to things of the fancy, but I dare say the majority of us find a pleasure and a relief in that heightened consciousness which is the result of the release of emotional conflicts in aesthetic activities.

It may, indeed, be said with some truth that in the last analysis the results of play and to a greater extent art are the most practical that can be obtained. Therefore the term 'practical' can only have meaning as a negative characteristic of play and art in its most common sense of material advantage. Thus defined, the practical is not a leading motive for either play or aesthetic activity.

III. THE SOCIAL REFERENCE OF AESTHETICS

(1) *Artistic production*

When we try to describe what seems to me to be an important difference between the two activities, we run into a seeming paradox which confuses the problem, the answer to which would otherwise be obvious. The difference I refer to is the social reference which I believe is necessary to art and not to play. This statement seems to be contrary to common experience. We not only play in groups but we prefer to do so. In fact, play is often given as an example of the so-called gregarious instinct. The child who is forced to play by himself is pitied because he has no companion to share his sport. Such a child is even handicapped, sometimes seriously, by his lack of social contacts which can usually be made so readily in play. On the other hand, the artist is depicted as a solitary creature who is frequently annoyed by the intrusion of friends. He is a person who often prefers to live alone, for the conflicts of his emotional life, which drive him into art, are the very experiences which so often drive him into solitude. The child in play usually wants to tell everyone near him what he is doing, and as for the adult in his lighter moods, one need only cite the golfer as an example of one who has an irrepressible urge to communicate the results of his activities to anyone who will listen to him. The artist too, obviously, desires the world to know what he has accomplished, but there are many exceptions which could be named of artists who have seemingly been totally indifferent to the external world—the extreme introvert who appears to revel in the concealment of his work. He is an individual who has been so hurt by criticism and censure that he prefers to hug the precious products of his imagination to his heart rather than to expose them to the comments of the cruel world. The artist is of all persons probably the greatest individualist, with the individualist's contempt for the opinion of others.

Yet in spite of these seeming contradictions, many aestheticians believe, and I think rightly, that, as I have stated above, social reference is the distinguishing feature of art as against play for the reason that it is an essential characteristic of art but not of play. In play one may often desire social response and approval. One may desire companions with whom to play, or to whom to recite one's achievements. Such companions, however, are not necessary; in fact they may be undesirable. Play is usually a product of the moment, and a desire for permanence seldom enters into the minds of the players. When it does and there is the added desire for social recognition, play seems to go over into art, as for example when the child who has been skipping around the room insists upon his parents' watching "how he does it"—a case of exhibitionism which is in a greater or less extent true of all art. We may say, then, that when play becomes acting, as it does when approval is desired, it crosses the borderline into the realm of art. There are, of course, exceptions, especially in cases where the play is in the form of competition which draws an audience. Here, obviously, there is no aesthetic quality, unless some members of the audience prefer to see only the formal and not the competitive aspect of the game. Games such as football and baseball have become institutionalized, and what was once a sport for the players frequently becomes work. I do not believe, however, that one can generalize. I am inclined to think that many players are interested in the game for the game's sake and that the audience plays a very secondary role, if any, in their consciousness. Certainly the same players who appear before a crowd are very willing to play without an audience for the pure sport of it, as is evidenced by the growth and popularity of intramural and interclub games in our colleges, where the audience is of minor importance.

As to the artist, in many cases the conscious desire for communication to an audience and approval by it are so obvious that examples are unnecessary. The question, however, is whether this social attitude is always present since, as we have stated above, he is likely to be antisocial in temperament. Many examples can be given of artists who have worked in seclusion and have expressed their contempt for either the approval or disapproval of society. Some artists even go so far as to destroy the results of their labour before any one has seen them, not because they were disappointed in what they had done but because they believed their work was of too high a quality to be appreciated by their contemporaries. Even when it is evident from the very nature of the work that it has been produced for a definite purpose, such as a Madonna for an altar-piece, and that it will undoubtedly have social significance, it is assumed by those with the highest ideals of art for art's sake that the painter's motive has been of the purest. They believe that he has been moved solely by the urge for self-expression, and that the only reward he looks forward to is the joy of a possible masterpiece. It is not difficult to believe that the artist's attention is entirely upon his work as he paints. We do not conceive of him as having his eyes on the canvas and his ears turned in the direction from which he hopes to hear the approval of the world. Yet I think that an artist, no matter how ideal he may be, at least subconsciously desires some social recognition. He may scorn the present generation as Philistines, but he hopes that a future age may recognize his genius and that the message he is trying to transmit from his inner self will not be altogether lost. He may in his idle moments sketch in sand, but if by chance he should produce something that took his fancy, he would hasten to place it upon canvas. The statement by W. Williams of men in general that "The prime influence on all of us to-day is our wish to enjoy the feeling of our worth as persons among other persons" (14)[1] is particularly true of artists. Whereas for most men, however, lack of adequate recognition may lead to an attitude of aggression, for the supersensitive artist the same lack of recognition makes for withdrawal from the world and a semblance of disdain for the very recognition which he seeks.

This desire for social contact and approval in artistic expression is seen among people of all ages and all civilizations and needs no elucidation. It is interesting to note, however, its emergence in primitive culture. The

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[1] Italics omitted.

African native woman who makes her own earthen jars is quick to seek praise for their beauty from a passing traveller, although her original purpose in their manufacture was a practical one. The primitive dance has its origin in religious, agrarian and martial ceremonies. It does not need a stretch of the imagination to see how such performances came eventually to be admired and then to be indulged in for their own sake. The cave drawings of Altamira were, according to some anthropologists, probably painted on the stone in the belief that the pictures of the animals would have the magical effect of procuring protection from and mastery over the animals thus depicted. We of to-day who admire their aesthetic quality cannot help believing that in the primitive mind of that early period, a feeling for their beauty mingled with a knowledge of their real purpose.

In literature we have very clear examples of the early development of the social aspect. The folk story probably owes its origin in many instances to a desire to develop the courage of the youth of the tribe, but as Bartlett has written, "The folk story is always to be regarded as primarily a social product, developed for and told to auditors, and consequently both as regards its form and its matter making, within its own community, a common emotional appeal" (2). The point is, then, that in the early life of both the individual and the race, the useful becomes the artistic through the addition of a peculiar social relationship.

(2) *Artistic appreciation*

Thus far we have been describing artistic production. A similar social relationship exists in artistic appreciation and distinguishes it from other judgements. In artistic appreciation we do not only want approval of ourselves as is the case in artistic expression, but we also desire agreement with our judgement which is very closely akin to a desire for such approval. Obviously the ego is very much involved in both cases. We want our companion to admire the picture that we particularly like, and the social communion in such common appreciation is part of our pleasure in the picture. If there is a disagreement we either suspect that there may be something wrong with our taste and therefore change it, or else we eagerly seek someone who will agree with us. Our pleasure in a symphony is somewhat diminished if our neighbours' response to the music is not sympathetic. Differences in aesthetic taste have even been of sufficient seriousness as to form the underlying cause of matrimonial difficulty.

We care about our non-aesthetic judgements, but not so vitally. If we find that our companion does not see the resemblance which we see between two mutual friends, we are disappointed but we are not irritated unless we have a serious inferiority complex. We like our companions to agree with us that the food we are eating tastes good, but we really do not care very much. Indeed, this desire for agreement has frequently been given as the reason why the experiences we get through the eye and ear are aesthetic as distinguished from those non-aesthetic experiences obtained through the so-called lower senses.

I need not dwell further on the fact of the fundamental social characteristic of art as disclosed in the desire for social approval. There is implied, of course, in this social relation a communication between the artist and his audience through aesthetic expression. It is this form of communication which I believe should be of at least as much interest to social psychologists as are language and gestures. It is frequently stated that in language and gestures the communication of ideas is the principal motive, in aesthetic expression that of affective and emotional states. Although this distinction is not a sharp one, it is in a general way true. There is little doubt that aesthetic expression is the best means for the social communication of affective states, and therefore one of the most fruitful sources for a study of such communication as well as for an examination of the states themselves—a fact which the psycho-analyst in particular has appreciated.

IV. THE PSYCHOLOGICAL BASIS OF AESTHETICS

(1) *Form*

Thus far I have dwelt upon the social nature of art. I shall now be more specific in regard to some of the psychological factors involved, for I believe not only that psychology is necessary in the analysis and understanding of the aesthetic response, but that the aesthetic field in turn offers some of the richest material for the exemplification of psychological principles.

Eugene Véron has stated "that art is the manifestation of emotion, obtaining external interpretation, now by expressive arrangements of line, form or colour, now by a series of gestures, sounds, or words governed by particular rhythmical cadence" (12). Tolstoy has expressed the same idea when he says, "To evoke in oneself a feeling one has once experienced, and having evoked it in oneself, then, by means of movements, lines, colours, sounds, or forms expressed in words, so to transmit that feeling that others may experience the same feeling—this is the activity of art" (11).[1] Both authors are, of course, referring to the formal factors in art. This manner of expression has, as is well known, been investigated by a number of psychologists. Lines are in themselves graceful, flowing and pleasing, or sharp, abrupt and unpleasant. Whether the observer gets the effect through empathy, as I believe, or through some kind of direct apprehension is beside the point. In painting and sculpture, the lines if executed by a talented artist will blend harmoniously with the subject-matter. If human figures are portrayed, the lines help to depict the quality of the personality as the artist feels it. It is hardly necessary to mention the fact that colours and sounds have abstract emotional effects. It should be added, however, that the effect of single units, such as a colour or a sound or a line, have little practical meaning for aesthetics. Here configurations are all-important. In fact, there is probably no field where the principle of configuration is better illustrated than in that of aesthetics, for unity is essential, a unity which is obtained through a balance of forces, a unity whose parts, so far as beauty is concerned, have no significance in themselves. This fact is obvious so far as music is concerned. It is evidently not so obvious in regard to colour, since we still speak of the aesthetic value of single colours. A colour which in itself might be unpleasant is made pleasing in combination, and where there is a conflict of a pair of colours the discord is resolved by the addition of a third colour.

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[1] Italics omitted.

By means of these formal elements alone the artist is able to express his own personality and often his deepest feelings. His work is indeed the best evidence for a response psychology. If it were not that he is able thus to reveal his inner self there would be little value in art. How far the artist is conscious at the time of the selection of specific forms to suit his meaning is in many cases difficult to tell. Undoubtedly very much of the process is purely subconscious. On the other hand, we have evidence from certain artists, like Van Gogh for example, that there is a deliberate choice of colour combinations to express definite moods and meaning. There has also been the attempt to express feelings by colour alone without form, as in the colour organ, or to combine colours and sounds which express the same moods. There has been partial success in such attempts. It is an open question, however, whether much beyond mood and the most generalized ideas can be expressed merely by form. Experiments have shown that meaning conveyed by music is very vague and must be largely supplemented by the programme. It is only rarely that the sounds of transmogrified words will convey unequivocal meaning, as Givler's work (5) has clearly shown. Moods and feelings are conveyed, but the sounds are unreliable conveyors of ideas. Gertrude Stein's pioneer literary venture is of particular interest to the psychologist in this connexion, because it shows the effect of an artist's transgression of fundamental psychological laws. Familiar words are used; but the mind is supposed to ignore the accustomed meaning and to get predominantly the effect of the sounds. It is like requiring one to perceive living persons as disembodied spirits. Obviously it would require more effort than most persons are willing to give to break up as strong a conditioned response as exists between words and their meanings.[1]

(2) *Suppression of meaning*

There is also the tendency to-day in the visual arts to convey an aesthetic impression by means only of formal elements. We find artists using various shapes and colours without a serious attempt at natural representation. Here again meaning is supposed to play an insignificant role, although it is the natural tendency, at least for the uninitiated, to look for meaning wherever possible. The psychologist knows that the firmly grounded habit of seeking the idea behind the symbol cannot be easily ignored. However, such attempts at art are so abstract that a meaning which may be read into it is of little help. For example, it does not add much to our aesthetic pleasure to be told that a few lines pointing to three coloured balls at the top of the picture represent ambition. It is not difficult, however, to understand the reason for such methods in the mind of modern artists. It is the well-known art for art's sake *leitmotif*—an imitation of the methods of music where one has a vehicle for free self-expression without the dangers of a wrong attitude on the part of the audience which meaning is so apt to introduce.

There is another form of self-expression in art which through its rugged individualism violates an important psychological law and thus loses its social significance. I refer to art which tries to communicate ideas as they are represented in the mind of the artist. What they give us are the images which happen to occur to them and, considering the nature of such images, it is not surprising that their representation should appear grotesque to most persons. The mind can consistently distort time and space without serious consequence, so long as one chooses to remain in the realm of images. The difficulty with such art is that images have merely a functional value so far as social communication is concerned. The actual images themselves are entirely individualistic and their nature irrelevant. What the social group is interested in and can understand is their meaning as behaviour. Being individualistic and arbitrary, they cannot serve as social symbols. Obviously it is for this reason that we have a conventionalized form of language. To take a classic example of the function of images, we have Titchener's visual image of a bald head as his personal, introspective symbol for the word 'but'. As a bit of descriptive psychology Titchener's statement has its place, but who would be particularly interested to see this symbol pictorially represented? The Surrealists have thought to put on canvas subconscious images, dreams, visions, automatic and psycho-analytic drawings. As a matter of curiosity, one may be interested for the moment in the picture of a watch bent limply over the limb of a tree, or the view of Western Pennsylvania edging into the harbour of New York, but who cares vitally? Art is supposed to offer means of escape and adjustment, but who desires to adjust oneself to the meaningless imagery of another's mind?

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[1] I understand the effect is better if one listens to Miss Stein's own rendition of her work, for she puts herself, as it were, into the sound of her voice, and one is thus better able to keep one's attention from the meaning of the words.

The above criticisms are not directed against self-expression, which is the very essence of art. The mark of individuality is a source of much of the pleasure that we obtain through art. It is this fact which gives us an answer to the question which Aristotle put and which has appeared so often since, as to why the ugly should be pleasing when it appears in the form of art. It is an expression of the self to the world through the medium of art which gives art its unique place in human affairs. But this expression must be intelligible and not in a form dictated by a solipsistic philosophy. As Dewey has stated, "...all art is a process of making the world a different place in which to live, and involves a phase of protest and of compensatory response. Such art as there is in these manifestations (of self-expression) lies in this factor. *It is owing to frustration in communication of meanings that the protest becomes arbitrary and the compensatory response wilfully eccentric*" (3).[1]

Art is a form of autistic thinking in which the subconscious desires and wishes forming the self find expression and become a pseudo-reality for the artist. It is not to be expected or desired that the expression should always be in conventional forms. Obviously there would be no progress in art if that were so, but just as autistic thinking may develop into the pathological states of schizophrenia or paranoia, which are distinctly antisocial, so may art, when pushed to the extreme, lose its social aspect.

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[1] Italics added.

(3) *Neglect of 'constancy'*

In pictorial art there is an interesting and legitimate departure by the artist from the manner in which his audience perceives reality, and, paradoxical as it may seem, it is only by this departure from the ordinary way of perceiving that the work can be properly comprehended. I refer to the artist's persistent effort consciously to neglect the law of constancy in his perception, so that his painting may seem 'natural' to his audience. The artist has had to train himself to see the world as it is rather than as it appears. The white paper at the window is painted very much lighter than the same paper at the back of the room, so that it may appear as of approximately the same brightness. Figures in the foreground are painted larger than those in the background in order that they may appear the same size. It was centuries before artists saw shadows unaffected by the law of constancy, and when they first painted them in the contrast purples, the audience was not prepared for the brilliance of the colours. Constancy was disturbed and the shadows seemed absurdly unnatural. I might add that there are situations where constancy has been kind to the artist and has helped him out of a difficult situation with his audience. An example is that of the effect of artificial illumination on the painting. The flesh tones of such paintings seem entirely natural under yellow artificial light, but the same tones seem an unduly saturated yellow when seen through a reduction screen.

(4) *The effect of social factors*

There is another form of constancy, operative in aesthetic judgements and matters of taste, a constancy which is akin to ultra-conservatism in public opinion. In no field, probably, do our imaginations and judgements show a greater lag through the effect of previous experience or are more strongly influenced by the social group than in aesthetics of industrial products. Mumford has said, "Form follows function, underlining it, crystallizing it, clarifying it, making it real to the eye" (9). Form, indeed, follows function, but unfortunately by very slow evolution. Seldom do we find a sudden emergence. The imagination is held in check by the previous form, as is probably best shown by the slow development of the shape of the automobile away from the old buggy, or the railroad train from the stage coach. Added to this inertia of the creator's imagination is the stubbornness of taste. Automobile makers, for example, had extreme streamlined designs in mind long before the present form appeared, but knowing their public they took advantage of a process akin to sensory adaptation and spread the alteration of lines over a period of years. In other words, the aesthetic response becomes conditioned to a certain configuration of stimuli, and it is by means of a gradual change in the configuration that the response is most easily changed.

The age of machinery has greatly hampered the expression of personality in the manufacture of utensils of everyday use. The majority of modern workers cannot indulge their aesthetic tastes in their daily work. In a way, however, Graham Wallas's wish uttered some years ago has come true. He said, "Streets would be pleasanter to live in if architects allowed more initiative to their assistants, and furniture would be pleasanter to use if skilled workmen were encouraged, at perhaps a slight increase of expense, to develop such opportunities of invention as are consistent with the use of machinery" (13). Artisans and artists have to-day invented forms in harmony with the machine and brought their designs in line with the present social culture. Houses, furniture and ornaments show the angles and planes of the machine-made products, rather than the easily flowing lines of the freely moving hand of the wood carver. And fine arts show the same influence in Cubism. The minds of the observers, as was stated, move slowly but they gradually take in the change and in turn reflect it in the taste of the period. Thus there is established the aesthetic response of groups, a response which distinguishes nations as well as individuals. As it affects the interplay of groups, it is one of the essential factors for consideration by the social psychologist.

Unfortunately art is not always allowed to develop freely, but it is restricted and at times even institutionalized by political influences. How far it is legitimate for art to be used for the purpose of propaganda is too large a question to be discussed here, except to say in general that when the aesthetic quality is uppermost in the mind of the artist, art is a legitimate vehicle for ideas of social value. It is when art becomes subservient to ulterior motives, as when a political leader forbids the representation of slim female forms as detrimental to the ideals of the state, that art is to be condemned by those who seek true beauty. However, art in the service of propaganda is a social phenomenon worthy of psychological study, and in doing so I believe we can share the optimism of Max Eastman when he states, "To my mind all this fervently noble plausible talk about art as a soldierly service to a cause, and this attempt at political dictatorships to produce the art they need by governmental word of order, is a temporary reaction to a development in the contrary direction. A splitting away of art from purposive enterprise, and from that understanding of factual relations upon which such enterprise must rest, is what really characterizes our age." (4).

It has not been possible here to describe all of the points of contact between aesthetics and social psychology, nor would it be worth while, since many of them, such as the social factors in the development of art forms and the reactions of the audience to the drama, radio and screen, are obvious. I have tried merely to show by a few examples not only that art is a means of social contact, but that it is the only means by which the individual can adequately express those innermost thoughts and feelings which are at the root of his personality. It is principally as a unique form of communication that I feel it should be included in the psychological study of society.

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AN EXPERIMENT ON HUMOUR

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I. INTRODUCTORY

THE experiment consisted of two parts. The first part, the presentation of material, was designed to discover if certain rules or tendencies operate when people laugh, and what they are. It was hoped by the reactions of subjects to sets of jokes, and by their introspection, to be able to classify types of humour and perhaps the types of people to whom certain sorts of humour appeal. It was hoped also to test the assumptions which have always been made by writers, that there is at least one common element in all things called humour, and that there is one experience common to all laughter situations. The experiment would either reconcile the numerous theories, or suggest new hypotheses, or disprove the assumption.

II. PRESENTATION OF MATERIAL

(1) *Method*

Fifty subjects took part in the experiment. This consisted of a set of thirty-two jokes, including three controls[1]; these thirty-two varied in nationality, age, propriety and in subject-matter generally. They were all translated into English. Twelve were pictorial and twenty written; of the latter some were read to the subject and some he read himself, the selection varying according to the subject's acquaintance with English and with the experimenter, and to any preference he might voice in the matter.

Half the subjects were shown the jokes from Nos. 1 to 32, and half in the reverse order. All the non-pictorial jokes were typed on uniform sheets of paper; the captions of the pictures were also typed and attached to the bottom of the pictures, in order to minimize the possibility of subjects' recognizing the source of a joke.

Apart from these efforts to ensure some uniformity, the conditions were as natural and unexperimental as possible. The experiment was usually preceded by a normal conversation, and throughout the subject was encouraged to talk, smoke, move and so on, as he pleased. The subject's mood at the beginning was noted (from introspection), and any information about himself which he or the experimenter thought important or unusual; for instance, his nationality and that of his parents, an account of his travels at home and abroad, his relation to the experimenter.

The subject was then given his instructions. He was told that not every number was meant to be funny; he was asked not to laugh if possible, and to mention if he had met any of the jokes before. All associations and imagery were to be reported and any sensation of tiredness or boredom which might assail him during the experiment. The subject was also requested to judge the degree of humour of each number, by placing each joke in one of the following categories:

Not funny (N), Slightly funny (S), Funny (F), Very funny (V), Extremely funny (E).

The reaction time for each number was taken, i.e. from the moment when the subject started looking at, or hearing the joke, till, having reached the end, he reacted in any way. Any sound or observable change of expression was counted as a reaction. When the subject had seen the thirty-two numbers, he was asked to introspect, with special reference to those which amused him most. He took these in turn, tried to explain why they stimulated laughter, and gave a fuller report of imagery and associations. Every remark made by the subject was taken down in as nearly as possible his own words. The experiment, including the discussion at the end, usually lasted about an hour and a half.

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[1] That is to say, pieces of prose material similar in form to the jokes, but not intended to be funny.

The majority of the subjects were English students, but there were nineteen Scotch, American, French, German, Italian, Indian and Japanese subjects. Eighty per cent. were between 20 and 30 years of age.

(2) *Disagreement*

Of the thirty-two jokes, every one was judged by at least three subjects as N; nineteen were judged by at least one subject as E (average no. of times these jokes were judged E = 4·2), and every joke that was judged both E and N was placed a minimum of four times in each of the other three categories. The laughter varied as much as the judgements: there was no joke which invariably produced a laugh; every one was received in silence at least once (average no. of silences per joke = 2·4). Amount of laughter was difficult to measure; the criteria used were loudness, length, spontaneity and infectiousness. In spite of the lack of unanimity, views were usually expressed forcibly and dogmatically. When jokes were placed in either of the extreme grades, the subject nearly always spoke with a high degree of certainty, not as if he were merely expressing his opinion, but as if the matter were unquestionably an objective one.

As was inevitable with the number of subjects used, a good many of the jokes were selected more than once as the best in the set. There was, however, as little agreement among these subjects as to why a number was funny, as there was as to which ones were funniest. Until they had got used to the idea of introspecting, subjects expressed their difficulties in the same sort of terms:

"I can't say why it's amusing, a thing either is funny or it isn't"; "Now I look at it again, I don't think it is funny"; "It spoils a joke to explain it", etc.; but once this stage is past, subjects have little in common. What appears to be a very simple joke is interpreted in two or three ways; different elements in a joke are extracted or emphasized as "the whole point". While one subject considers that his answer is complete if he merely says "the picture here is funny", another may go into an elaborate analysis of the humour contained in the caption, holding that the picture serves merely to summarize the situation.

Answers fall roughly into three classes:

(1) *Explanation.* The subject speaks as though asked to explain the joke to a child or a foreigner; for instance, he tells what the picture represents, or states the convention upon whose breaking the point of the joke depends.

(2) *Reference.* The joke, or part of the joke, reminds the subject of something which categorically is funny; it may be another joke, or a certain person or situation, or the joke presented may evoke an image which is funny. Such answers of course merely shelve the question, but from the frequency with which they occur in one form or another we are forced to conclude that certain entities as such are funny.

(3) *Analysis.* The subject tries to analyse the form of the joke. Such answers are either:

(*a*) Objective: the most commonly given being incongruity, self-contradiction, anticlimax, disproportion between—or reversal of—means and end, or

(*b*) Subjective: for instance, superiority (within or outside the joke), variation in tension, admittance to consciousness of ideas normally repressed.

Sometimes the subject's answer is rather a criterion of the quality of a joke than an analysis of its form. These criteria often go in strangely contradictory pairs, e.g. surprise and expectation fulfilled, plausibility and improbability, simplicity and complexity.

(3) *Judgements*

Subjects understood immediately when asked to judge the degree of humour of each joke; in fact a good many began by objecting to the experiment before the instructions were given, by saying that they often did not laugh when considerably amused, and conversely. They were not pressed to grade a joke, however, when they found difficulty in so doing: if

(1) they insisted that 'funny' in any degree is the wrong word, that the joke is for instance 'clever' or 'witty';

(2) they could not judge it as a whole: a few numbers consisted of several parts;

(3) they did not see the point.

Jokes judged by the subject to be futile attempts at humour, or to be controls, were placed indifferently in the N category.

In Fig. 1 are shown the judgements and the amounts of laughter which each joke received. In obtaining the figures, each E judgement was counted as four units, each V as three, etc., and the laughs were

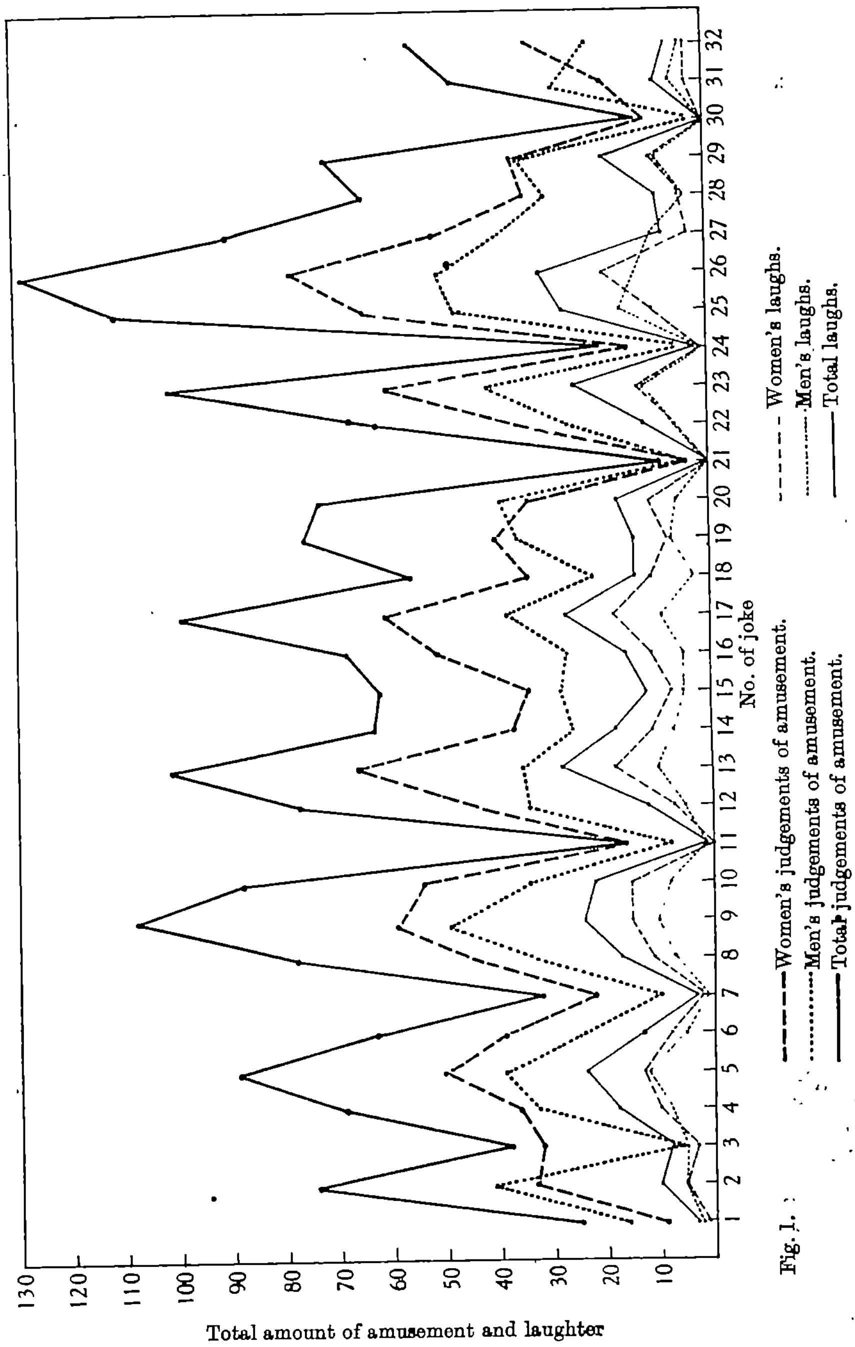

Total amount of amusement and laughter
130
120
110
100
90
80
70
60
50
40
30
20
10
No. of joke
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
31
32
Women's judgements of amusement.
Men's judgements of amusement.
Total judgements of amusement.
Women's laughs.
Men's laughs.
Total laughs.

Fig. 1.

similarly divided into four classes. These curves show a fairly accurate correspondence except for jokes 9, 27, 32, 14 and 18. In the case of the two latter, we see that the judgements are slightly lower than would be expected from the amount of laughter; the first three were graded more highly than the laughter which they produced would justify. These three jokes contained more wit and satire than the other numbers in the series.

However, it would be risky to generalize from the correlation shown in this experiment for the majority of the jokes, since most subjects gave the impression of exercising a greater caution when giving their judgements than they would in a normal situation.

(4) *Time lag*

The timing was virtually useless, since the differences depended much more on the rate of reading of the subject than on the time he took to 'see' the joke. Also, a number of subjects gave no sign at all, until they made a remark which showed clearly that they had seen the point some time ago. However, it was possible to ascertain some points about the time lag between finishing a joke and 'seeing' it. There is evidently a maximum such time lag up to which amusement increases. This is contrary to the generally accepted idea that the more spontaneous the laughter the greater the amusement. The additional laugh is very likely not purely amusement; it is probably partly a laugh of pleasure and relief at having at last seen the joke, and a signal to others that the joke has now been seen. In some cases the joke may have been particularly subtle and therefore took longer to see and was more amusing than the average joke (subtlety was often given as a criterion of a good joke). There does, however, appear to be a kind of storing-up process. Just as laughter is always louder and amusement greater if for some reason it has been necessary consciously to control one's mirth, so also perhaps some analogous process takes place in the unconscious while we are struggling to see the point of a joke. In so far as it is possible to deal with such matters in quantitative terms, it looks as if the sum of amusement at two jokes—when for some reason the first produced no laughter—is usually greater than the addition of the two laughs.

The length of the maximum pause naturally varies with individuals and situations. In the experiment it was on an average about 5 sec. Beyond this maximum little or no laughter was aroused, and subjects usually graded such jokes as N or S.

(5) *Pictorial and written jokes*

There was a tendency for subjects with vivid visual imagery to prefer written jokes, heard and read, to pictorial ones. They presumably found their own images more satisfying than the pictures given; sometimes the former conflicted with the latter and the subject would say "the joke is quite good but would have been excellent with a different picture". On the other hand, subjects with little or no visual imagery were liable to prefer the pictures.

Several similar experiments with different jokes and subjects must be carried out before attempting to draw conclusions as to the relative amusement aroused by pictorial and written jokes. This one experiment shows no significant difference: the average number of laughs per picture = 14·08 and per written joke = 16·17 (counting every laugh as one unit, irrespective of length, loudness, etc.; when allowance is made for these, the averages are respectively 19·75 and 23·47).

The difference measured by judgements is still smaller, and the E's and V's contradict each other in this respect:

Average number of E's per picture = 2·6, per written = 2·8.
" V's " = 9·25, " = 8·4.

The controls, all of which were written, are not included in these figures. They were, except by one Communist and one Japanese, graded low and scarcely laughed at (see Fig. 1; Controls were Nos. 11, 21 and 30). The most usual reaction to the controls was a puzzled frown and a second reading, followed by some remark from the subject indicating that he was feeling very dense—this, in spite of having been told at the beginning of the experiment that not all the numbers were meant to be funny.

(6) *Nationality*

The universal belief in the existence and importance of variations in sense of humour according to nationality was repeatedly confirmed during the experiment, both by the frequency with which subjects gratuitously and correctly stated the origin of a joke, and by the constant recurrence of the same mistakes. English and American jokes were nearly always recognized; French ones were sometimes thought to be American. German and French jokes were generally designated as 'Continental'; there is evidently, as would be expected, a greater difference between English and Continental humour than between the various Continental types of humour. An Italian subject, when explaining what seemed to him the difference between English and Italian humour, said, "You see, in Italy our jokes always have a point." If that is the case, the type of joke so popular in England and America, the humour of which depends entirely on the fact that the joke is pointless, would have no acceptance in Italy, however well translated. The impossibility of translating jokes is based on something more than the numerous meanings and associations connected with the actual words of the original joke; it is part of the essence of humour.

Hypotheses as to the differences between nationalities are clearly not permissible at this stage. Until we have some idea of the tendencies at work among individuals with the same background, we cannot begin to measure, and estimate the significance of, differences between groups.

(7) *Sex*

Twenty-one of the subjects were men and twenty-nine were women. The curves in Fig. 1 suggest a few tentative conclusions with regard to differences of sense of humour between the sexes. The experiment indicated a surprisingly high degree of agreement between men and women as to the quality of the humour of the jokes judging both by laughter and by grading, but the women appear in both respects to be more extreme: their peaks are higher and their dips are lower.

The pair of judgement graphs cross each other only four times, and these crosses are due to the fact that jokes Nos. 3, 20 and 31 were rated more highly by men than by women (though No. 20 was more laughed at by the latter than the former). Nos. 3 and 31 were political and No. 20 was 'pointless'. The pair of laughter graphs show five crosses: Nos. 3, 11, 25 and 27 were the only jokes which aroused more laughter from men than from women. No. 25 (the most popular of all among men subjects) ended with a parson's swearing forcibly, and No. 27 was a *Punch* joke; No. 11 was a control, a cutting from *The Times* which concerned Göring and which evoked very little laughter from either sex, except from a male Communist.

The high correlation of amusement between men and women shown in the experiment is probably untrustworthy. Everyone undoubtedly believes that fundamental and universal differences exist, but it is naturally one of the most difficult branches of humour on which to obtain data: men assume, and dogmatically assert, that jokes which they tell among themselves do not amuse women, and vice versa, and thus neither sex gives the other an opportunity of testing their convictions. There was one joke, No. 24, a French pictorial one, of which several men said "this does not amuse me, but it would probably appeal to women"; several women said of the same joke "I don't find this funny, but I expect it would appeal to a man". In fact, as the figure shows, few subjects of either sex laughed at it.

It would be interesting to see whether an experiment carried out with the same set of jokes by an experimenter of the other sex would produce significantly different results.

(8) *General*

Apart from the fact that it is risky to draw conclusions from one set of subjects and of jokes—the experiment has at least shown the impossibility of one individual's accounting for objective differences (if such exist) in the goodness of jokes—there are obvious objections to applying experimental procedure to a study of humour.

All the usual drawbacks to experimental conditions such as the effect of suggestion, change in what is to be observed as the result of observation, variation in mood of experimenter and so on, are present, and some are augmented; for instance, introspection of amusement is notoriously one of the hardest and most untrustworthy kinds of introspection, and the social element in laughter is perhaps its most essential factor. It seems probable that the number and kind of companions of the laughing individual determine his reaction, to a far greater extent than does the joke which he thinks is stimulating his laughter. The reading aloud of some jokes was meant to make some allowance for the immense importance which everyone attaches to the telling of a joke. This proved fruitless: the experimenter failed to realize that the important point is *how* a joke is told, not whether it is heard or read. Thus in order to ensure uniformity the jokes read aloud were narrated in as neutral and colourless a manner as possible (it would obviously be impossible to tell a joke for the fiftieth time in the same way as for the first time unless this plan were adopted). There was therefore practically no difference in the average amount of amusement at heard or at read jokes; in fact that for the latter was slightly higher. One or two subjects, however, did laugh at the humourless way in which the spoken jokes were told.

The self-conscious attitude and artificial atmosphere inevitably present can be dispelled during the course of the experiment only to some extent, since the presence of the stop-watch, the repeated request for a judgement and the hurried scribbling of the experimenter continue till the end. Furthermore the joke is but a small and relatively unimportant part of the huge realm which we label 'humour', though it is no doubt the easiest on which to experiment.

There are, on the other hand, one or two advantages in the experimental procedure: the laugh of politeness is almost entirely eliminated, since the subjects feel for the most part that it is not offensive to give no sign of amusement at an 'experimental joke' and also since they were asked not to laugh if possible. One or two subjects did in fact laugh slightly and then say interestedly, "I don't really think that's at all funny, you just get into the habit of laughing as soon as anyone tells you a joke". Polite laughter apparently plays a still larger part in the hearing of jokes than is generally supposed; it evidently actually arouses an experience of amusement on occasions.

Thirty-two jokes on end sounds too much, but only two subjects asked for a pause in the middle, in spite of the instructions. In whichever order the jokes were taken the tendency was rather to 'warm up' than to lose interest towards the end.

Finally, the experiment throws a certain amount of light on the jokes used, for if a subject does laugh or smile with any appearance of genuineness in the conditions described, there can be little doubt that he is considerably amused, and at the joke itself.

III. RECALL OF MATERIAL

(1) *Method*

Twenty-four weeks after the first subject had been tested, all subjects who had taken part in the experiment were requested to write out as many of the jokes as they could remember, in the order in which they remembered them. They were asked too to report which jokes, if any, they had repeated and how these had been received.

Of the fifty who took part in the original experiment thirty-eight also did the recall. The maximum length of time interval was 24 weeks, and the minimum a fortnight. The recalled jokes may be divided into three classes:

(1) C, the original joke is remembered completely and correctly,

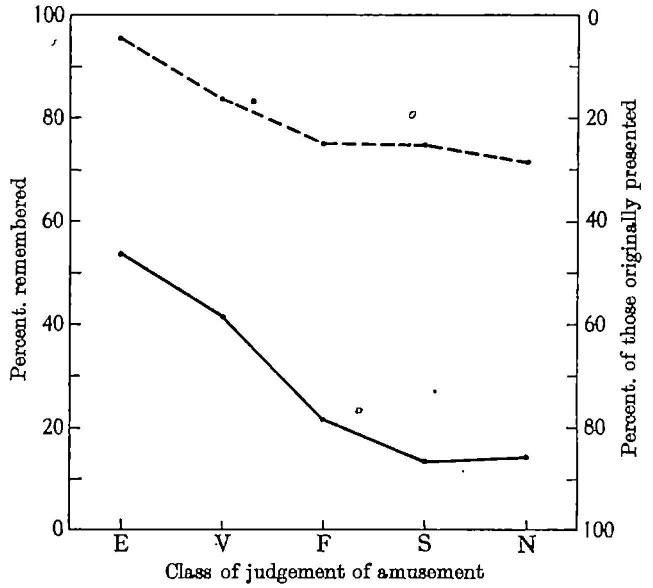
(2) I, the subject has the right idea, but alters, or shortens, or changes the emphasis, etc.,

(3) M, it is just possible to see which joke the subject means.

A few subjects sent in a list of titles without writing out the jokes in full. These were usually classified as I, except in those cases when the subject showed through some accompanying remarks that he clearly remembered them.

(2) *Amusement and memory*

(*a*) *Laughter*. The total number of jokes remembered by the thirty-eight subjects = 271, 22·8 per cent. of the total presented. But the number of jokes which both had been laughed at and were remembered = 204 and the total number of jokes laughed at = 326, therefore the percentage of laughed-at jokes which were remembered = 61·96. Alternatively, the percentage of original jokes which produced laughter = 26·8, while the percentage of remembered jokes which had produced laughter = 75·27. It would thus appear that there is a high correlation between amount of laughter at a joke and capacity to recall it.



—— Percent. of each class of judgement remembered.
----- Percent. of total number of originally presented jokes placed in each class of judgement.

Fig. 2.

(In obtaining these figures, every laugh irrespective of length or loudness, and every joke remembered, whether C, I or M, was counted as one unit.)

(*b*) *Judgements*. The correlation between amount of amusement, as measured by judgements, and capacity to recall is shown most clearly in Fig. 2.

The continuous line shows the percentages of each judgement which was remembered, e.g. 53·7 per cent. of the E. judgements were remembered. Thus the lower the grade, the smaller the proportion of jokes remembered, except for the N judgements which show a slight rise. This is to be expected, since a joke which is strikingly 'unfunny' is more liable to impress itself on a subject than one which very slightly amuses him. (In this graph again C = I = M.) The dotted line shows the distribution of judgements of the original thirty-two jokes, e.g. 25 per cent. were judged F. The scale of this curve is inverted, as the correlation is a negative one. The most frequent judgement remembered is E; the most frequent judgement made is N.

(3) *Time interval and memory*

The total number of jokes remembered = 271. This figure naturally does not include jokes which subjects mistakenly included in their reports. Thus the average number remembered per subject = 7·13. Only one subject remembered none, with time interval = 24 weeks; the highest number remembered = 15, with time-interval = 2 weeks—the only subject who did the recall after less than 6 weeks.

In Fig. 3 is shown the average number of jokes remembered (C = I = M) for the different time intervals, e.g. subjects who wrote their reports between 21 and 25 weeks after doing the experiment remembered on an average 5·9 jokes. This curve shows a low correlation between length of time interval and number remembered, since it rises to 8·6 for 11–15 weeks and sinks again to 7·4 for 6–10 weeks. The significance of the steep rise for the fortnight can clearly not be estimated, since it is obtained from one subject only. However, we are tempted to suggest that the importance of the time-interval length decreases as a geometrical rather than an arithmetical progression.

There does appear, however, to be a correlation between length of time interval and *accuracy* of recall: the curve for the average number of jokes correctly and completely remembered (C) shows a rise as the time interval is shortened, even excluding the 2 weeks' subject. The sudden dip at 20–16 weeks is caused by the fact that two subjects at this interval had no C's. On the other hand, the curves for I and M both have a downward tendency, the latter to a greater extent than the former.

It was clear from subjects' reports as to which jokes they had repeated, and also from a study of their remarks during and immediately after the experiment, that these factors play a more important role in determining which jokes shall be remembered than does length of time interval.

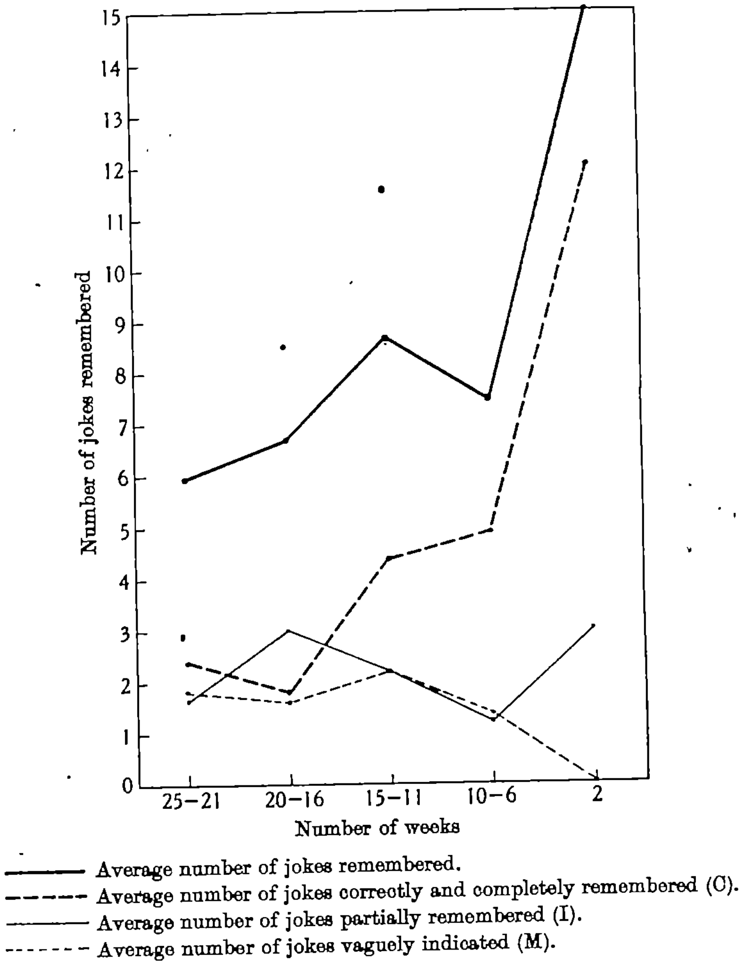


Fig. 3.

(4) *General*

Mistakes made in the reports were very varied. A description of a joke with the point omitted occurred quite frequently, even from subjects who had appreciated it; and there were a few cases of substitution. Eight jokes were included which had not been presented originally; half of these had at some time been discussed by the subject and experimenter. A number of subjects condensed two or more of the experimental jokes, thus altering the trend of the originals, and sometimes achieving a less clear but far more amusing story. Condensations were usually effected between jokes which had at least some superficial resemblance in subject-matter. In a number of cases the subject gave in his report the associations which he had described in the experiment, under the impression that he was repeating the original joke.

There was only one case of remembering a written joke as a picture and none of the converse. No greater facility was shown in recalling pictures or stories: the ratio of pictures to written jokes in the experiment = 0·6, while the ratio of pictures to written jokes which were remembered = 0·77. There appeared to be no correlation either between subjective certainty, or priority in order remembered, and objective accuracy.

IV. CONCLUSIONS

The main result of the experiment is to make one exceedingly chary of setting up any rules on the subject of humour, and to allay the great temptation of classifying types of humour and senses of humour. At the present stage it looks as if this would merely involve a listing of individual jokes and individual people. The numerous variations shown are not, I think, just a natural result of the limited scope of the enquiry; an informal discussion on the subject always leaves one with the same impression of lack of objectivity.

On the other hand, certain specific problems have presented themselves together with hypotheses as to their solution. The experimental method must be supplemented by observation, both of individuals and of groups of varying sizes and composition, in social situations. However, experimentation has at least demonstrated the futility of forming plausible and attractive theories based on unverified assumptions.

I wish to express my thanks to Prof. Bartlett for his advice and help in the experiment.

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LEARNING AS DOING: SOME SUGGESTIONS CONCERNING THE ANALYSIS OF DIRECTION

By W. LINE and W. R. WEES

(*University of Toronto*)

I. *Introductory discussion* (pp. 162–164).
II. *Experimental illustration* (pp. 164–167).
III. *Relation to previous work* (pp. 167–169).

I. INTRODUCTORY DISCUSSION

Learning is doing. This is an abstract statement. Concretely, to learn something is to do it. To think of learning as doing, is not to think of it as having (or as 'getting'). One may do 'it' in order to have 'it'; but he cannot do and have the same 'it'. To this rule there is one exception. One may do a job and have a job, 'job' being the object of the verb in each case. But to have a job is to have something to do, and we are returned to the activity.

If thinking of learning as doing denies the thought of learning as having, then in thinking of learning as doing we cannot use the various psychological concepts which have grown out of the thought of learning as having. Of these, the most trenchant has been the concept of retention. On the basis of a 'doing' postulate, 'retention' has no meaning; for, to retain is to have, and to have we are denied if in learning we are to do.

Further, to think of learning as doing is not to think of it in the passive voice. In learning we do, we are not done by. Hence in our thinking we are denied the assumptions of motivation, on the one hand, and of effect on the other. We cannot be motivated, nor can we be affected; for to be moved or affected is not to do, but to be done by.

Denied the 'laws' of retention, motivation and effect, what have we left? Primarily, we have *activity*. To do is to be active. Activity, therefore, is our basic postulate. Granted movement, one requires no mover for 'motivation'; and needing no mover, he does not need to think of the mover's 'effect'. Neither does he need to think of 'retention'; for activity is not retained, but maintained, and the maintenance is provided by our postulate.

The analysis of activity is secured by means of the second postulate derived from the notion of learning as doing. Used as a finite verb, 'doing' is transitive. There is an *object* of the activity: "I do *it*." Hence, our postulated activity is qualified always by *direction*, and direction is defined (in part) by the object of the activity.

But the postulated object of the activity ties direction only at one end, so to speak; and direction defined only at one end is not defined. Definition at the other end is provided by the *subject*: "*I* do it." Activity thus defined in direction, by the subject as one terminus and by the object as the other terminus, becomes amenable to analysis.[1]

Analysis may proceed, theoretically, either by way of the subject or by way of the object, since activity itself cannot be treated except as it is qualified by direction. Attempts to analyse activity in itself have usually been led from 'doing' to 'having' in order to secure substance for analysis. The value of thinking of activity in terms of its directions, defined by subject and object, lies in the fact that, in direction, kinds of activity are easily distinguished.

Distinctive features occur by way of changes in direction. To study changes in direction one must be able to fix a point either of subject or of object, and observe variations in the other. If both change at the same time, direction cannot be secured. Let us fix the subject terminus. Two terms for the subject terminus are frequently employed to suggest the continuing subject: one is the 'self', the other is the 'individual'. The former term is used for the 'subject regarded by one'; the latter is used for the 'subject regarded by many'. The latter way of looking at the subject is the better for experimental purposes because the subject regarded by one cannot be examined scientifically. The private, inscrutable character of the 'self' is expressed in the words by which 'self' is qualified: 'my' self, 'him' self and so on. The only way in which the self (the subject regarded by one) can be treated experimentally is to turn the self into the individual (the subject regarded by many). We might as well, therefore, start with the individual in the first place.

The difficulty in starting with the individual as the fixed point in the study of direction is to know his object. In any relation between subject and object, as in "I do it", the object is quite as subjective as the subject. The subject we have decided to regard from the point of view of the many. Can the object be so regarded also? Not in the present tense. But in the past tense the activity turns into accomplishment, and the accomplishment may be regarded by the many.

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[1] It should be noted that the subject is not the 'mover' who moves something, or who is moved; neither does the subject act on his object. The subject is active, or—better—the subject-object relationship is always dynamic.

Let us refer to the regard of the many as the objective regard. Objectively, the individual and his accomplishment may be related by the activity as in the sentence, "He has done it." We may now study the qualities of 'it'. We may compare this 'it', as accomplishment, with the accomplishments of other individuals, or with the accomplishments of the same individual at different times. One accomplishment, we may decide, is 'different from' or 'better than' another, provided that we are able to establish suitable criteria of difference and of value.

But we are not confined to measurements of differences or to standards of improvement. We are able to make subjective inferences from the objective regard. If we socialize the activity by providing a task, we may say to the individual, "Do this." He does 'it'. When he has done 'it', we may compare the 'it' of the accomplishment with the 'this' of the task, and from what was done infer the way in which it was done. By the 'way' we refer to the direction. That is, in the 'way' we are returned, by inference, to the subjective regard, a point of view from which we were prohibited from making observations. We are returned, also, to the present tense, and may conclude, "This is the way he does it."

We have thus described a procedure for detailing the directions pursued in the learning process, because in so far as we may discover the directions of the activity, in so far may we differentiate between activities. Observing the accomplishment (or, in the case of accomplished movement, the performance) we infer what the object was, regarded subjectively; and knowing the object regarded subjectively, we know the way.

If, now, we call the object regarded subjectively the 'end' (meaning 'goal', perhaps), our procedure provides us with knowledge of ends and ways. The practical value of knowing ends and ways lies in the fact that knowing them we may implement them.

II. EXPERIMENTAL ILLUSTRATION

To illustrate the procedure for discovering ends and ways, we may briefly describe an experiment in which children were given the task of reproducing a prose passage which they had read. If we think of the requirements of a task in terms of the words, "Do this," then in the case of the reproduction, the 'this' was what was read, and the doing was the writing of it. By comparing what the children wrote with what they read, we may discover the children's aim in the task's accomplishment; and knowing the aim, we may find the way in which the objective is achieved.

In this experiment, the aim (the 'end' of the activity) is inferred to be conformity—i.e. to write a story (narrative or exposition) as nearly as possible like the presentation. This inference regarding the aim is drawn from the following considerations: The reproductions of simple stories were, in each case, a whole piece. Something started at the beginning was finished at the end. If the children found, while reading over their work, that an idea had been omitted, they inserted it in its proper place—they did not tack it on at the end. Matter for the reproductions was seldom, if ever, drawn from outside the presentation. And though, as versions of the presentation, the reproductions were considerably contracted, yet ideas from the presentation were selected and written in a way such that the whole reproduction should conform to the given story.

Conformity having been inferred as the aim, it was important to know the way in which the children secured it. Experimental means to find the way seemed to lie in a further character of the reproductions, the character of divergence. It was observed that though each reproduction conformed to the presentation, yet each child wrote his reproduction in his own manner. In other words, the reproductions showed individual divergence in the ways in which the same story was expressed. With this observation to start from, it was thought that if the children could be induced, by some form of presentation, to diverge, not individually, but as a group, a reason for the divergence might be discovered. Knowing the way in which divergence came about, one could, by converse, infer the way in which conformity was produced.

To induce uniform divergence in place of conformity, several forms of presentation were used. The character of one form was such that an idea expressed at the beginning of the passage was left incomplete, and was followed, as in a rambling discourse, by ideas which ran off at tangents to those immediately preceding. In another paragraph, two themes were presented only one of which was completed at the end. In still another form, the end of the story was placed foremost, and the children were led through the end to the beginning. Six forms of like nature were used—forms in which a direction laid down at the beginning was not sustained in the ideas that followed.

In their reproductions of these passages, the children diverged from the presentation with a considerable degree of uniformity. The character of the divergence was as follows: (i) If an idea was left incomplete in the presentation, the children, in their reproductions, finished it. Similarly, if the conclusion of the passage was inapt, the children changed it to make it suitable. (ii) If the direction offered by an idea in the presentation was seen at a new angle in a subsequent idea, the latter was changed to accord with the direction already accepted. If a direction was accepted in the middle of a passage, antecedent ideas were changed, in reproduction, to suit the new direction. (iii) Ideas in the presentation which the children perceived as inconsequential to each other were made consequential in the reproductions.

These three ways of describing the changes which the children made have as their common factor the concept of direction. In the reproductions a direction is laid down, and is sustained by the provision of a complement, a consequence, or a redirection. And since direction is defined by termini (the point of departure and the end), the act of achieving sustained direction may be called *determination.*

It will be observed that in the word 'determination' we have made an inference regarding the way in which the act of reproduction of a prose passage occurs. From the determined character of the reproductions we have inferred that children accept a point of departure and an end, and that from beginning to end an acceptable direction is pursued. But in the case of the forms of presentation from which the inference was drawn the children distorted the presentation; they did not conform to it. By converse, however, we may conclude that in the case of a presentation to which they do conform, they accept the direction offered by the beginning of the passage, they perceive the end to be a complement of the beginning, and between the beginning and the end they apprehend the relation of one item to another as one of consequence.

From the point of view of practice the question may be asked, what must be the character of a form such that the child will accept the given direction and sustain it? The answer appears obvious. In order that activity should be pursued in a given direction, the beginning of the presentation must be perceived as incomplete. As incomplete it inspires an inquiry. Further to the passage, intermediate items should sustain the inquiry, and the end should satisfy it. Since, in its original meaning, another word for inquiry is 'story', we may say that a form likely to be reproduced with conformity is one qualified by 'story value'.[1]

Our experimental procedure has thus provided us with a means of defining both the end of the activity and the way in which the activity occurs, when the task is that of reproducing prose passages. The end is conformity, and the way may be called determination. Since these inferences are taken from the accomplishment and are referred to the experience of the subject, we have been led to a definition of the subject-object relation. Such definition, we have argued earlier, could not be reached by way of self observing the experience of self; for in this experience the latter 'self' becomes the individual; not 'the' self, but 'a' self. 'A' self, however, is only one individual, and one person is an inadequate sample. In that fact lies the fault of conclusions from empirical data.

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[1] Again we would emphasize that the concepts here used do not involve notions of motivation. The subject-object relationship is itself dynamic—and its dynamics are here made more meaningful by such terms as 'inquiry', 'determination', and the like. The 'story' is not conceived as motivating the child through interest or appeal to 'basic motives'; the story is determinable by the child in his apprehension of it.

As an extension of the empirical procedure, introspection suffers from the same difficulty. But in introspection the fault is aggravated; for, as empiricism is the inferential description of the experience of a person, introspection is but the description of the description of a person. And though the investigator may use the introspections of several people, he does not alter the fact that his inferences are twice removed from the experience to which they are referred. For our procedure it is claimed that: (i) examination of the accomplishments of a group of people provides inferences directly referable to the experience of the subject; (ii) by the examination of further accomplishments executed in tasks constructed on the basis of the inferences, the latter may be tested to any required degree; (iii) based on the principles of experience, definite prediction of the quality of accomplishment in a learning task is possible; all that is required is that the task should be examined for the degree to which it is determinable by the subject.

III. RELATION TO PREVIOUS WORK

So far we have presented in outline a point of view which seems likely to be fruitful in attacking problems of learning. We have indicated something regarding the method implied in that point of view, and have described briefly some investigations directed by the method. There remains to us here the task of relating the foregoing to current contributions along similar lines.

Our method will immediately be recognized as growing out of Bartlett's recent work on *Remembering.*[1] In that work, the author protests very forcibly and effectively against the traditional insistence upon memory as content, and claims for consciousness an active role. This activity is frequently disregarded, particularly by those who concentrate their attention upon the ideas or elements retained, or who consider recall processes solely in terms of the activity of the elements which, pertaining to past experience, mysteriously come to mind and function in the present. Reactivation of past experience—in the crude sense implied by elementarist theories—is an untenable, because illogical, doctrine, according to Bartlett. He therefore takes a bold step in the direction of a vague terminology—as did Lashley, among others—and of a less definitely controlled experimentation.

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[1] Bartlett, F. C. (1932). *Remembering.* Cambridge University Press.

Bartlett's argument and methodology seem to be identical with our own, particularly up to the point where 'inferences' are made regarding the 'way' of understanding or remembering. His inferences, however, appear to rest quite heavily on the past (on previously formed schemata), despite his insistence on an active consciousness. Consciousness, for him, selects from past schemata, organizes them for future or immediate use; and we seem thus to be involved in the systematic and experimental difficulties of a retained past opposed to a dynamic, forward-looking present.

The Berlin Gestalt school, and in America the stimulating writings of Wheeler, appear to avoid this particular set of difficulties. But in insisting upon the uniqueness of the whole—whether the whole in question be the complete act of behaviour or the phenomenal field cognized—these contributions make the task of scientific analysis, even on the functional side, extremely vague, if not impossible. It seems to us that by suggesting a possible analysis in terms of the 'end', we have preserved the unity demanded by *Gestalttheorie*, without in any way inhibiting the experimental investigation of 'process' or 'activity' and without subsuming all mental function under the undifferentiated law of Gestalt. Unity is preserved by our basic postulate of activity, interpreted in terms of the subject-object relationship. Process analysis and description of different forms of function are secured by our concept of direction, analysed in terms of the objects subjectively regarded. In this way we have endeavoured to combine the fundamental achievement of Ward with the experimentally directed insight of Spearman.

The latter statement calls for amplification. Ward's conversion of Brentano's act-psychology from an intangible doctrine of pure process to a more readily observable and therefore more meaningful theory of subject-object interdependence, was deficient mainly from the point of view of a clearly indicated method of experimentation. As we have suggested, the work of Bartlett supplied the experimental complement to a degree. But the contribution of Spearman is even more pertinent at this stage of our argument, in that the doctrine of noegenesis is the direct result of scientific examination of the dynamics involved within the subject-object relationship itself. This doctrine depicts the essential form assumed by *activity as process*. All that we have added is a suggestion as to how the *direction* of that activity may be further described and experimentally investigated.

To give emphasis to this addition, we may conclude by stating our experimental approach in the following terms, which could readily be revised to show that it was in an exactly similar way that Spearman arrived at his analysis of process: The difficulties of so-called 'objective' approaches to learning, and those of traditional approaches through 'experience', have been very great. The first observes the learner's performance of what (to the observer) appears to be the same task when done on several occasions. Since repeated performances frequently show progression in time taken, the interpretation is that repetition produces greater efficiency by facilitation. This is no explanation of the observed phenomena until further inferences are made concerning synaptic conductivity or the like—inferences which can never be checked (at least at present) by further observation. The second, trying to observe from the learner's point of view, sees the task as changing with each performance; and hence 'repetition', in a purist's sense, is impossible. But the observer, being the learner, always stands in a unique relationship to the experiment. Hence scientific control is denied. We wish to combine the advantages of scientific observation with the availability of the experience of the learner. By framing hypotheses as the result of observing tasks—hypotheses or inferences concerning what we have called 'the way' the learner does the task—we may check the hypotheses by implementing tasks such that the inference will be tested to any required degree. This, we assert, the experimentation quoted in the middle section of this paper illustrates in regard to the concept of *direction*. Spearman's work on the laws of cognition, examined by means of his techniques in the field of individual differences, similarly illustrates it in regard to *process*.

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A STUDY OF GESTALT PROBLEMS IN COMPLETED AND INTERRUPTED TASKS

PART III

BY A. R. PACHAURI

I. *Individual differences in the U–C effect* (pp. 170–171).
II. *Description of the tests* (pp. 171–175).
III. *The intercorrelations* (pp. 175–177).
IV. *Consideration of results* (pp. 177–178).
  (*a*) *The U–C tests amongst themselves* (p. 177).
  (*b*) *Correlation of U–C tests with 'P'* (pp. 177–178).
  (*c*) *Other correlations* (p. 178).
V. *Conclusions* (p. 179).
References (pp. 179–180).

I. INDIVIDUAL DIFFERENCES IN THE U–C EFFECT

THE important problem that confronts us is that of throwing some light on the question of 'individual differences' in the U–C effect as referred to at the end of Part I of this paper (1). We have already drawn attention to the fact that this question has so far received no proper consideration. In all the investigations on the U–C effect hitherto, the number of subjects and especially the psychological range covered by the experiments has been rather small; it seemed desirable therefore to extend in both directions and obtain a broader basis of fact.

Experimental work on the psychology of individual differences of the U–C effect can take two courses: (1) an attempt might be made to obtain correlations between U–C tests themselves for a fairly big group of subjects, and (2) an attempt might be made to establish a possible contact between the U–C effect and any other psychological factor or factors that are likely to be connected with it.

The first course does not lend itself to satisfactory investigation since the repetition of U–C tests with the same group of subjects results in a considerable deterioration of the U–C effect as we have observed already (2). Consequently stable correlations cannot in general be established between the U–C tests. If two U–C tests follow each other closely, the effect is marred in the second test.

The second course affords ample experimental opportunities for a comparative study. Firstly, the U–C effect appears to have an 'inertial' character; and already two broad psychological factors, also of an inertial kind, have been isolated. These are (1) the factor '*P*', that of so-called general inertia yet appearing to cover ideo-motor activities only (3), and (2) that of ideational inertia or fluency. Secondly, the connexion, if any, between the U–C effect and '*W*'—which has been explained as "persistence of motive"—obviously requires examination. Thirdly, although the U–C score is so arranged as to cancel or equal memory abilities, any score such as U/C is always open to correlative influence between U/C and either U or C, so that control of the U–C effect could well be made in terms of independent memory tests. These factors, then, together with Spearman's general intelligence factor '*G*', could well be used for the said purpose of a comparative study.

The main object of our research is to determine what connexions, if any, these factors have with the U–C effect; whether the effect under discussion is explained already by some of the above factors, or whether an essentially new factor is indicated for the U–C effect.

II. DESCRIPTION OF THE TESTS

To pursue the second course introduced above, we conducted a preliminary experiment with a group of 80 boys and girls, consisting of an equal number of each (age 13–14 years). There were three groups; each was given three periods of testing, the same conditions of experimentation being employed for the three groups.

The tests applied are given in Table I below. The table shows (i) the main factors tested, and (ii) the tests used for the measurement of these factors respectively.

Table I

The main factors tested	Tests applied
U–C effect	(1) Miscellaneous U–C test A
	(2) Miscellaneous U–C test B
	(3) Opposites test
	(4) Synonyms test (same letter)
	(5) Synonyms test (different letter)
'*P*'	(6) 'SSS'
	(7) 'ZZZ'
	(8) '999'
Fluency ('*F*') (4)	(9) Free association
	(10) Ink blots
	(11) Controlled association (the data for this was obtained from Miscellaneous U–C test A)
'*G*'	(12) Pairs
	(13) Analogies
'Memory'	(14) Kelley's Memory test
'*W*' estimates	(15) Estimates by two responsible teachers for each group

Table II shows the order in which these tests were applied; (I) gives the period; (II) the test; and (III) the time required for application.

Table II

I	II		III
Period	Test no.	Tests in order of application	Time allotted for application in min.
First	12	Pairs	10
	1	Miscellaneous U–C test A	30
	13	Analogies	10
	9	Free association	5
Second	3	Opposites	15
	10	Ink blots	8
	4	Synonyms (same letter)	15
	6	'S'	15
	2	Miscellaneous U–C test B	30
Third	5	Synonyms (different letter)	15
	14	'Memory'	45
	7	'Z'	15
	8	'9'	15

Detailed descriptions of the various tests are given in the following section.

The U–C tests

The U–C tests were of two kinds, Miscellaneous A and B respectively, and the Synonyms and Opposites tests 3, 4 and 5:

Miscellaneous tests. A full description of the complete tests A and B has been given previously. They have already been used in the course of the work described in Part II of this paper ((2), pp. 448–53). Miscellaneous U–C test consists of miscellaneous tasks, all of a verbal nature, each requiring a minute or so, for completion or interruption. The tests were applied as described in Part II ((2), p. 451). The test B is a second form of the test A: it consists of the same kind of tasks, and was applied in the same way as test A.

For comparative purposes, we used Lewin's variable, namely the ratio of U to C for tests A and B combined and test A alone which was applied first. The latter alone was used in our later investigations, since the two tests (A and B) correlated with each other very poorly ($r=0{\cdot}092$), a result which we may ascribe to the influence of repetition. This is also confirmed by a low correlation between the U scores of the two tests ($r=0{\cdot}095$). We have already observed in Table III, Part II ((2), p. 456), that the value of U/C for test B is only 1·5. It is apparent that the U–C effect is almost destroyed for reliable individual differences on second testing. We propose to call this isolated variable for test A, var. 1.

In using two forms of the Miscellaneous test, we hoped to study the effect of repetition of U–C tests; for reasons mentioned above, the U–C test first applied (Miscellaneous U–C test A) can be used as the crucial test for correlation with the variables.

The Synonyms and Opposites tests. We have dealt with these tests in detail in Part II, Exp. IV, under the heading of "Difficulty of tasks" ((2), p. 453), and we have also observed that the U–C effect does not ensue under such circumstances. Yet we applied these tests again to give them further trial, by making the time limit for each item shorter than before (8 sec. as compared with 15 sec.), in order to minimize the chances of memorizing an 'easy' item in the surplus time after solving it. The present device did not improve the main effect as indicated by the results shown in Table II, Part II (2), p. 455), the value of U/C for the three tests, in order of application being 1·2, 1·2 and 1·0 respectively.

'*P*' *tests*

Three '*P*' tests were applied, (*a*) the 'SSS' test, (*b*) the 'ZZZ' test, and (*c*) the '999' test.

The tests were applied with a special technique suggested by Dr Stephenson. Each test had its own especially prepared printed sheet. In each case, a sample letter was given, and the subjects were required to write letters of this shape and size in the lines provided. The aim is to secure some approach to standardization of the work done in '*P*' tests. The subjects are not to make exact copies of the given letters, but have to write as quickly as possible, every letter being as nearly as possible the same as the given letter in size and shape. Taking the 'SSS' test as a sample, the test had the following parts:

(*a*) 30 sec. writing 'SSS'
(*b*) „ „ 'SSS'
(*c*) „ „ 'ƧƧƧ'
(*d*) „ „ 'SƧSƧ'

Two sample letters are allowed as a preface to each part; a rest pause of 10 sec. approximately, intersperses each part.

The instructions take the following form, a sample being in front of the class, on a black-board: "You have to write ordinary S's as big as those given, and with the same amount of spacing. The letters should all be clearly and well written; all like those printed. You are not to spend too much time on each letter. You have to write as many of the letters as you can in 30 sec. Although you write as many as you can, each must be well written. That is, you have to write as *quickly*, but also as *well* as you can. When I say 'Go!' you have to start writing but not before I say so. As I say 'Stop!' you must stop at once. It is very important to do just what I tell you to do, and to start and stop when I say so."

The instructions for writing reversed 'S' and 'S', in its alternate form, were given in the same fashion.

The mean of the first two activities (writing ordinary 'S' for periods of 10 sec. each) was denoted by X, and the mean of the reversed and alternate forms of writing 'S' was denoted by Y. The '*P*' score for the individual was obtained from the formula, $'P' = X/Y$.

The three '*P*' tests, scored as above, correlate by a mean amount 0·22, which compares well with the results obtained by previous workers (Wynn Jones (3), Pinard (5), Stephenson (6), Sivaprakasam (7)) with normal subjects as experiment. Without entering in detail into problems about the validation of the '*P*' score, both as a factor of measurement and as one with inertial quality as its explanation, we propose using the composite '*P*' score given by the sum of the '*P*' scores (on a standard basis) for the three tests, as the invariable representing '*P*' factor.

The Fluency tests

Three tests were used as a measurement of 'fluency': (i) Free or uncontrolled association; (ii) Controlled association, (iii) Ink blots.

(i) *Free association.* The test is described in Whipple's Manual ((8), p. 410), and has been used by Pyle (9), Wynn Jones (3), Hargreaves (10), Sivaprakasam (7) and others as an '*F*' test. The subjects are supplied with pencil and paper, and the following instructions are given: "I want you to write different words as fast as you can. You may give any words you like but they must not run into sentences or clauses. Again, you must avoid writing the names of objects in the room, and the same word should not be repeated. I shall tell you when to stop."

In scoring this test, no account is taken of quality of response, the number of words written in 3 min. being the crude score.

(ii) *Controlled association.* This test too, is described in Whipple's Manual ((8), p. 437), and is of wide use as a test of '*F*'. In the present case, three of the C items in the Miscellaneous U–C test A were used, as follows:

No. 3. Name any countries that you know.

No. 11. Write down words consisting of three letters only.

No. 15. Name any things that are green or might be green.

The time allotted for each of these three items was uniform for each group of subjects. After the lapse of the allotted time, the tasks terminated, in each case with the remark, "That will do", from the experimenter.

(iii) *Ink blots.* The test is well known ((8), pp. 620–6) and was originally used by Wynn Jones (3) as a measure of ideational '*F*'.

To make the test applicable as a group test, we prepared small booklets each containing five ink blots. The ink blots were numbered, and 1 min. was allowed for each. The instructions were: "Every one of you has a set of 5 odd-shaped ink blots. I shall ask you to take them in order from 1 to 5, one at a time, and to write down on the numbered blank the things you can see in each blot. Try each blot in different positions. These blots are not real pictures of anything, but I want to see if you can imagine pictures of things in them, just as you sometimes try to see figures in the fire or clouds. Do not try to turn over before the word is given, and do not look back again."

The three '*F*' tests provide scores which correlate by a mean amount 0·39. Their correlation with '*P*' is negligible (see Table V). We propose, again without attempting any further validation of these '*F*' tests as a measure of an '*F*' factor, to use the composite '*F*' score as a variable for correlational work.

'*G*' *tests*

Two non-verbal tests were used as a measure of '*G*' factor. Recent work (11) shows that these non-verbal tests supply a measure of pure '*G*' factor. The test no. 12, "Paired abstraction", has already been described by Brown and Stephenson (12) and the Analogies test, no. 13, by Burt (13). In both cases we used the tests supplied us by Dr Stephenson.

The two '*G*' tests yield scores which correlate by a mean amount of 0·24. The '*G*' scores require no factor validation, and we again use the pooled score as our measure of '*G*' factor.

Memory test

The test was essentially the same as that applied by Kelley ((14), p. 154). It consisted in exposing a card for a few seconds containing separate short words which the subjects of experiment had to memorise; one such card is that shown below:

Sample card no. 5
DO
BE
NO
US

To measure the immediate memory, the subjects were then required to turn to an answer sheet where, in a particular column, were to be found the words they had just seen on card no. 5. Any words remembered had to be cancelled in the answer sheet from the column mentioned by the experimenter. Altogether the answer sheet contained 42 rows of 8 words each; there were also 42 exposure cards. The cards were exposed in irregular order (nos. 5, 15, 41, 13, ... etc. in order of application). Each card was exposed for 9 sec., so simplifying the test applied by Kelley who used varying times for exposure as well as cancellation.

'W' estimates

We collected careful estimates as to the character qualities of persistence of the three groups of children that composed the population (15).

For each group, two teachers independently assigned marks to the individuals entrusted to them, for four persistence qualities, namely, perseverance, reliability, trustworthiness and amenability. The highest mark a subject could get for any quality was +3, and the lowest −3. The marks thus allotted to each individual were added up, and then the 81 subjects ranked according to the total. It may, however, be mentioned that the teachers making the estimates knew nothing of the results of the experimental tests, which at the time were not even worked out; and the experimenter did not receive the result of the estimates until he had completely finished his own task of marking and correlating his tests.

III. THE INTERCORRELATIONS

In the first place we could find no evidence for sexual differences in the U–C effect; the mean U/C for the girls was 1·84 and for the boys was 2·0. We therefore put all together for correlation purposes.

We have already concluded that repetition of a U–C test mars the U–C effect, reducing the ratio, for instance, from 1·9 to 1·5. But the reduction is apparently still worse for individual differences, since the correlation between U/C for the Miscellaneous tests A and B is only 0·09.

Nevertheless, we must remember that, apart from any special significance for recall of particular items in test B, the individuals who score highest for test B have done so in spite of a fore-knowledge of the required recall. They are likely, therefore, to be the most interesting individuals for the purpose of a study of individual differences. We found that the respective U and C scores and U/C, for tests A and B, gave much the same correlations respectively with the various variables we have to consider (see Table V); hence it was considered feasible to use in the main correlation table composite U and C scores for the two Miscellaneous tests A and B, instead of only the scores for A, or for A and B separately. The variables 1, 2 and 3 in Table III below, then, are for composite scores for tests A and B.

Although we have seen that the Synonyms and Opposites tests, involving very short-time tasks, failed to give a very significant U–C effect, we include them, for comparative purposes, in the main table of correlations. The variables 4 and 5 below are for the composite scores derived from the three Synonyms and Opposites U–C tests (nos. 3, 4 and 5 of Table I).

We have therefore the variables shown in Table III for one main correlation table for this verbal group test experiment.

Table III

(1) U for Tests A and B (Miscellaneous)
(2) C ,, ,, ,,
(3) U/C ,, ,, ,,
(4) U for the tests 3, 4 and 5 (Synonyms and Opposites)
(5) C ,, ,, ,,
(6) U/C ,, ,, ,,
(7) 'Memory—test no. 14
(8) '*G*' sum of tests no. 12 and 13
(9) '*F*' sum of tests no. 9, 10 and 11
(10) '*P*' sum of tests no. 6, 7 and 8
(11) '*W*' estimates—test no. 15

The correlations for these variables are shown in Table IV, and the subsidiary Table V shows the corresponding correlations for U, C and U/C of the separate A and B (Miscellaneous U–C tests). The probable error for zero correlation for this population is 0·07, so that all correlations of about 0·20 are over three times probable error value, and may be tentatively accepted as significant.

Table IV

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1)	—	·41	·52	·198	-·013	·058	·223	·262	·232	·058	·087
(2)	—	—	-·17	·093	·168	·03	·241	·223	·134	-·182	·269
(3)	—	—	—	·12	-·004	·134	·008	·123	·194	·204	·035
(4)	—	—	—	—	·302	·18	·095	·110	·284	·017	·09
(5)	—	—	—	—	—	-·21	·111	·09	·395	-·230	·05
(6)	—	—	—	—	—	—	·07	·16	·144	·083	·065
(7)	—	—	—	—	—	—	—	·353	·246	-·181	-·033
(8)	—	—	—	—	—	—	—	—	·394	·025	·092
(9)	—	—	—	—	—	—	—	—	—	-·081	·082
(10)	—	—	—	—	—	—	—	—	—	—	-·089
(11)	—	—	—	—	—	—	—	—	—	—	—

Table V

	'*G*'	'*F*'	'*P*'	'Memory'	'*W*'
U (Miscellaneous A)	–·09	·084	·22	·27	·14
C ,,	·04	·13	–·12	·23	·17
U/C ,,	·07	·124	·261	·069	·068
U (Miscellaneous B)	·05	·18	·19	·423	·14
C ,,	·03	·172	–·05	·31	·07
U/C ,,	·041	·12	·14	·08	·10

IV. CONSIDERATION OF RESULTS

(a) *The U–C tests amongst themselves*

The correlations between U and C for the two sets of tests, Miscellaneous, and Synonyms and Opposites respectively, are $r_{1,2}=0{\cdot}415$ and $r_{4,5}=0{\cdot}302$. Both U and C for the Miscellaneous tests correlate equally with 'Memory' (0·223 and 0·241 respectively); whilst the U and C scores for the Synonyms and Opposites correlate only 0·095 and 0·111 with the 'Memory' score. In both cases, however, the Lewin score U/C reduces the correlations with 'Memory' to 0·008 and 0·07 respectively. We may, therefore, use the variables 3 and 6 as representative of U–C effect, independent of 'Memory' as such.

The 'cross' correlations for the two sets of U–C tests must now receive attention. The correlations are $r_{1,4}$ and $r_{2,5}$ for the correlation between Miscellaneous U scores and Synonyms and Opposites U score and between Miscellaneous C score and Synonyms and Opposites C score respectively. The values are 0·198 and 0·168 respectively; they are small, but 'Memory' does not explain the values, since the partial correlations $r_{1,4}$ and $r_{2,5}$ are not much less (0·18 and 0·15 respectively) than the first correlations. The two types of tests, then, appear to work in the same direction, slight though the correlations are.

(b) *Correlation of U–C tests with 'P'*

The C scores for the Miscellaneous, and Synonyms and Opposites tests respectively correlate negatively and apparently just significantly with '*P*', the values being $-0{\cdot}182$, and $-0{\cdot}230$ respectively for $r_{2,10}$ and $r_{5,10}$ respectively.

On the other hand, U scores for the same two types of tests correlate $+0{\cdot}058$ and $+0{\cdot}017$ respectively with '*P*'. Now U and C correlate positively, and if there should be any significant psychological effect represented in these scores, then we wish to obtain the *negative* correlation of C with '*P*', whilst that of U with '*P*' (though only suggestive) is *positive*.

We therefore made a slightly more exhaustive examination of these correlations with '*P*'. For the Miscellaneous test A alone, we find that $r_{\mathrm{U}, 'P'} = 0{\cdot}22$ and $r_{\mathrm{C}, 'P'} = -0{\cdot}12$ (Table V).

It is known that, amongst school children, those estimated as *low* for '*W*' (*i.e.* of poor persistence and character qualities) have extremes of '*P*'. We selected the nineteen cases of lowest '*W*' in the group and found that $r_{\mathrm{U}, 'P'}$ and $r_{\mathrm{C}, 'P'}$ were now 0·505 and −0·381 respectively. These are now significant values, the one *positive* as before and the other *negative*. Again, U and C for these nineteen cases correlated 0·18.

If, apart from the influence of 'Memory', the persistence properties of U correlated with '*P*'; it might well correlate significantly under conditions of experiment slightly better than those used above. Then it would be expected that C would correlate negatively with '*P*'; for the very individuals giving most U, give least C, in virtue of any significant persistent effect of U.

We notice that all the other variables, '*G*', '*W*', '*F*', as well as 'Memory', correlate positively with the U and C scores, only $r_{\mathrm{C}, 'P'}$ correlations are negative.

Further, '*P*' and 'Memory' correlate negatively (−0·181); so that the above suggestions of correlation between '*P*' and U are not attributable to 'Memory'. It is perhaps difficult to understand why 'Memory' should correlate negatively with '*P*', especially in view of the law of Memory given by Spearman ((16), p. 291); but further work is required before much can be said on this question.

In these correlations with '*P*', then, we see some reason at least for further experimentation.

(*c*) *Other correlations*

We find that the fluency scores correlate equally on the average and positively with the U and C scores. U/C correlates 0·194 with '*F*' which is as high as the value 0·204 between U/C and '*P*'. But fluency does not show the anomaly described above, for '*P*'; further, it is probable, that a general speeded activity factor (the 'S' of Studman's work (4)) spreads over U, C, fluency and 'Memory'. '*W*', we find, makes no significant correlation with any variable except Miscellaneous C score—a result so far inexplicable except on chance grounds. But the correlations for '*W*' are rarely linear and, as shown by the above-mentioned data in which the nineteen cases of lowest '*W*' were selected, the '*W*' estimates may allow of the segregation of individuals of extreme '*P*', for whom correlation with U–C tests might be more clearly observed.

V. CONCLUSIONS

Although suggestive results have been obtained, the above experiment has been of a preliminary nature only. We need not therefore draw theoretical conclusions until confirmatory and better results have been obtained. Our main result, however, concerns the correlations of U and C scores with '*P*' under conditions that would fit with a theory that '*P*' and the U score have much in common, if the latter is freed from the effect of 'Memory'.

But the correlations of U and C with fluency require further examination; we see that 'Memory' is a useful control, and that it correlates negatively with '*P*'. The positive correlation of 'Memory' with other variables, '*G*', '*F*', U, C, however, is consistent with its negative correlation with '*P*', since '*P*' is scored on in inverse scale to these other variations. In these correlations it is difficult to see the role of 'Memory', but possibly a general 'activity' factor covers all.

We suggest, then, a repetition of the experiment, an attempt being made to improve the U–C and other tests. This can be done particularly for the '*P*' tests, whilst it is doubtful whether any more satisfactory U–C test is easily obtainable. We might, however:

(i) secure a more reliable measure of '*P*' by increasing the number of tests applied for the measure of '*P*';

(ii) secure a more pronounced U measure, by taking into consideration only those tasks that are recalled before hesitation. This can be achieved by directing the subjects to draw a line across their recall sheets, when a long pause occurs at the time of recall. Only those tasks that are written above the line would be taken into consideration for scoring, to obtain the value of U/C for each individual prior to hesitation.

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THE RELATION BETWEEN THE AMPLITUDE OF OSCILLATIONS IN SHORT-PERIOD EFFICIENCY AND STEADINESS OF CHARACTER

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I. Introduction

In everyday life it is common to classify people roughly as 'reliable' and 'steady', or as 'unreliable' and 'erratic', according as they carry out their work over years of acquaintance in a consistent manner (which may be either well or poorly), or else do it well at some times and badly at others without any apparent cause of change.

It seemed possible that the amplitude of a person's oscillations in continuous working over short periods might be connected in some way with his steadiness of character. To investigate this four tests of oscillation were arranged, and two independent estimates of steadiness of character were obtained for the subjects used.

II. Description of tests used

Two of the tests were mainly of motor abilities, the first consisting of printing the capital letters of the alphabet continuously, the second of writing the cardinal numbers continuously. The other two tests involved cognitive processes to a greater extent, the first being to pick out from a series of pairs of digits those pairs where the sum of the digits was ten, the second to pick out from another series those pairs where the difference of the digits was three.

III. DISCUSSION OF PREVIOUS RESULTS

These four tests were used to give a measure of the amplitude of oscillation for each subject. Experiments with reversible figures have also been used to give a measure of the amplitude of oscillation, but with inconsistent results from different investigators (2). In an earlier investigation Flugel (1) obtained curves of output and measured the amounts of oscillation in individuals. He used the same material (adding sheets) throughout a long series of tests with school-children. In order to eliminate errors due to a rising or falling work curve Flugel summed the changes from one period to the next and took the average change as a measure of oscillation, instead of taking deviations from the mean. In the present investigation the work curves do not show any general tendency to rise or fall (see Fig. 1), and this method has not been adopted.

Flugel also found that the fastest workers oscillated more absolutely, but less relatively than the slow ones, and the present results agree with this.

A further result of interest is that Flugel found that oscillations during one day's tests correlated significantly with oscillations from day to day. The latter would give an estimate of oscillation for periods between the short periods used in the present tests, and the long periods necessary to estimate steadiness of character.

IV. DETAILS OF PROCEDURE

The present tests were carried out in Govan Secondary School, Glasgow, in June 1934. In order to have a sufficiently large number of cases two classes were used, the total number of children being seventy. Through absence or other causes, the number which completed the tests was fifty-five. A larger number could not be used owing to the nature of the investigation: If a connexion between oscillation in the tests and steadiness of character as estimated by a teacher was to be obtained, the same teacher for all the pupils was needed. Further, to obtain a check by means of another teacher's estimate, it was necessary for *two* teachers to have the same two classes which were being used. This is unusual in a secondary school, and the writer was fortunate in that two of his classes were taken by the same teacher for another subject, and so could be used in the tests.

One class was of 1st Form girls, average age 13 years, 2 months, the other of 2nd Form girls, average age 13 years, 9 months. As the tests were given in June, the estimates of character were made after at least 10 months' daily acquaintance with the children.

The tests were all carried out during school hours, and were entered into with considerable zest—their progress was marked by that intense silence which is characteristic of a class absorbed. There did not appear to be any need of an external incentive to make the children work their hardest; they were simply told that the tests were to find the best and steadiest workers, and therefore they were to work as hard as they could.

The subjects made a vertical stroke at the place reached on the paper every time the writer called out 'Stroke'. This was done every 15 sec. for 5 min., giving twenty periods. Separate days for each test were used.

In the digits tests, because of the irregular occurrence of the special pairs of digits on the paper it was not possible for the subjects simply to cross them out and ignore the others. Each pair would tend to take the same time to examine whether the sum of the digits was ten or not, and so there would have seemed to be less work done in a 15-sec. period with few special pairs than in one with a few more. The subjects were therefore asked to cross out the special pairs by a stroke ／ and the other pairs by a stroke ＼. All rubbers were removed to prevent disturbance of the results by time lost in their use. A pair crossed out wrongly, if noticed, was to be corrected by a further stroke, giving ×. Very few cases of this were actually found.

V. METHOD OF USING THE TEST RESULTS

It was found by graphing the work done per period against time that there was no appreciable tendency for improvement or fatigue to occur (see Fig. 1). This held both for individuals and for the group as a whole. Hence it was not necessary to adopt any method of compensating for a rising or falling work curve.

The most obvious criterion of the amplitude of oscillation is the standard deviation of the amounts of work done per period. This was worked out in each case. Inspection of the figures indicated that the fast workers had larger standard deviations than the slow ones. The correlation between standard deviation and speed (mean score) was worked out and found to be $r = 0{\cdot}35$, P.E. $= 0{\cdot}074$. Hence the standard deviation could not be used to measure the amplitude of oscillation, being largely dependent on speed.

To eliminate the effect of speed, the standard deviation divided by

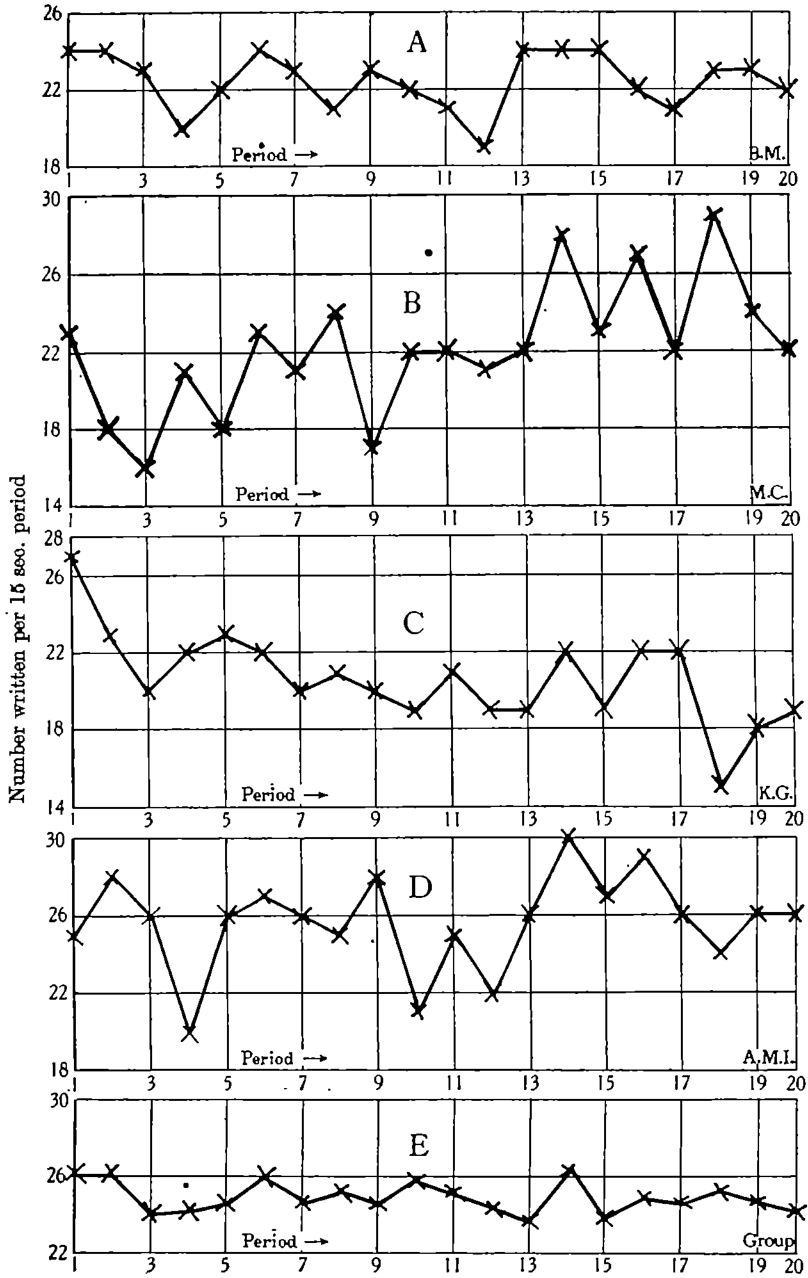

Fig. 1. Work curves for writing cardinal numbers. *A*, *B*, *C*, *D*: Specimens of individual curves. *E*: Composite curve giving group average for the 55 subjects.

the mean score was tried as a measure of the amplitude of oscillation. When correlated with the mean score this gave $r = -0{\cdot}12$, P.E. $= 0{\cdot}090$. Hence the $\frac{\text{Standard deviation}}{\text{Mean score}}\left(\frac{\sigma}{M}\right)$ was taken as a measure of the amplitude of a subject's oscillation.

Using this criterion the correlations shown in Table I between the results of the four tests were obtained:

Table I

r	Adding digits	Subtracting digits	Writing letters	Writing numbers
Adding digits	+*0·72* (P.E. *0·06*)	+0·54 (P.E. 0·06)	−0·21 (P.E. 0·09)	+0·005 (P.E. 0·09)
Subtracting digits	+0·55 (P.E. 0·06)	+*0·51* (P.E. *0·06*)	−0·09 (P.E. 0·09)	−0·05 (P.E. 0·09)
Writing letters	−0·21 (P.E. 0·09)	−0·08 (P.E. 0·09)	+*0·45* (P.E. *0·07*)	+0·41 (P.E. 0·07)
Writing numbers	−0·03 (P.E. 0·09)	−0·06 (P.E. 0·09)	+0·42 (P.E. 0·07)	+*0·19* (P.E. *0·09*)

To obtain an indication of the self-consistency of the tests σ/M was worked out for the odd periods of work and also for the even periods, and the two sets of values correlated. These figures are given in the leading diagonal of the above table in italics. The figures in the upper right half of the table are the observed values, those in the lower left half the values when age is eliminated.

VI. DISCUSSION OF CORRELATIONS OBTAINED

The average value of the coefficients is 0·22, but only two are significant, namely those between writing letters and writing numbers, and between adding digits and subtracting digits. They are in marked contrast to the correlations obtained by Flugel in testing eighty children (2), where he found the intercorrelations of eight tests to be all positive, and the correlations between two similar tests to be no higher than those between any other two. The tests used by Flugel were all similar in that they all involved discrimination and crossing out, whereas the two writing tests given to my subjects were almost entirely of motor ability, requiring no discrimination.

Flugel also reports, however (2), that the presence of a single factor for oscillations in sensory thresholds, reaction times, and mental work is not established conclusively. Puroshottam and Shivaprakasam (quoted in (2)) did not find such high correlation between tests of oscillation in mental work and oscillation of thresholds, or reaction times, as between the latter themselves. This therefore is in agreement with the writer's results above.

Further consideration of the individual measures of oscillation showed a tendency for the older children to have smaller amplitudes than the younger. The mean measure of amplitude for the four tests when correlated with age gave $r = -0{\cdot}33$ with a P.E. $= 0{\cdot}08$, which is significant. Chen found a similar result (quoted in (2)) for younger children, but a positive correlation for older children (ages not given). The writer's result only applies to the range from 12 to 14½ years.

On account of the low correlation between the writing and arithmetic tests, the correlations between age and the mean writing amplitude, and between age and the mean digits amplitude, were worked out separately. They gave:

Mean writing amplitude and age: $r = 0{\cdot}003$, P.E. $= 0{\cdot}090$.
Mean digits amplitude and age: $r = -0{\cdot}39$, P.E. $= 0{\cdot}075$.

VII. ESTIMATION OF STEADINESS AND ITS CONNEXION WITH TEST RESULTS

The other side of the investigation has now to be discussed—the estimation of a person's steadiness of character over long periods. Some of the subjects were judged after two sessions' acquaintance, the others after one session.

Estimates of steadiness were given according to five classes, A, B, C, D, E, and each child was judged in the following way:

"In assigning a mark for the degree of steadiness or unsteadiness consider whether the child *inclines* one way or the other from the average. If so, give B or D. If not, give C. If the child is very markedly steady or unsteady give A or E."

The two sets of estimates obtained independently in the above way were found to give a mean square contingency coefficient of $C = 0{\cdot}55$, which is of the same order as those for various moral qualities mentioned by Burt (3).

In attempting to find a numerical relation between the test results and the estimates of steadiness, the test figures were grouped into five classes to correspond with the estimates. In doing this A was given to those with the smallest amplitude of oscillation. Hence any tendency for low oscillation in the tests to be connected with steadiness as estimated would give a positive coefficient of connexion.

The mean square contingency coefficients between the test results and the estimates were found to be as shown in Table II.

Table II

Contingency coefficient for ...	Estimate I	P.E.	Estimate II	P.E.	Mean estimate	P.E.
Mean oscillation (4 tests) and estimate	0·51	0·08	0·40	0·10	0·75	0·05
Mean writing oscillation and estimate	0·53	0·08	0·35	0·10	0·76	0·05
Mean digits oscillation and estimate	0·50	0·09	0·58	0·08	0·74	0·05

The magnitudes of these coefficients are again similar to those mentioned by Burt for the connexion between moral qualities and independent estimates.

As shown previously, the writing test results do not correlate significantly with age and so the figures given above may be taken to show a significant connexion between oscillation in the tests and steadiness of character. For the arithmetic figures, however, allowance must be made in view of the fact that the oscillation in arithmetic correlates significantly with age.

Also the estimates of steadiness are themselves positively connected with age as follows:

Estimate I	Age and estimate: $C=0{\cdot}41$.
Estimate II	Age and estimate: $C=0{\cdot}55$.

When the effect of age is eliminated the coefficients from Table II become those in Table III (the previous values in brackets):

Table III

Contingency coefficient for ...	Estimate I	Estimate II	Mean estimate
Mean writing oscillation and estimate	0·59 (0·53)	0·44 (0·35)	0·85 (0·76)
Mean digits oscillation and estimate	0·40 (0·50)	0·47 (0·58)	0·68 (0·74)

These values, although low, are all of the same order of probability, from 3½ to 7 times their probable errors, and indicate the presence of a connexion between oscillation manifested in short periods and steadiness of character. But although showing a connexion the tests are not sufficiently valid to be of much prognostic value, as would be required by tests forming part of a comprehensive test of character or temperament. It is noticeable however that the connexion is considerably increased when the mean of the estimates is taken, and this suggests that it might be found to be still greater if a larger number of estimates were obtained and pooled. The biggest drawback to this kind of investigation is probably the coarse grading of the estimates. A numerical estimate is wanted, but is difficult to get. Some investigators, e.g. Webb (4), have used seven classes in estimating various qualities of character, but even with five it was sometimes found difficult to place a child; to use more than five did not seem feasible.

VIII. SUMMARY

1. Four tests of oscillation were given to fifty-five subjects. Two were of writing, two of arithmetical processes. Two independent estimates of steadiness of character were obtained.

2. The two writing tests were significantly correlated, and so were the two arithmetic ones. The other intercorrelations were not significant.

3. The arithmetic tests correlated significantly with age (negatively) but the writing ones did not.

4. The contingency coefficients between the estimates of steadiness and the oscillations shown by the tests were of the same order (0·4–0·5) for both arithmetic and writing. This held for both estimates, when the effect of age was allowed for.

5. The contingency coefficients between the mean of the estimates and the oscillation shown by the tests are higher (0·7–0·8) and suggest that a bigger number of estimates might be used with advantage.

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THE VARIATION OF SENSORY THRESHOLDS WITH THE RATE OF APPLICATION OF THE STIMULUS

II. TOUCH AND PAIN

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I. *Introduction* (pp. 189–190).
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I. INTRODUCTION

IN a previous paper (Grindley, 1936) the writer has described measurements of the variation of the differential threshold for pressure with the rate of change of pressure, and has discussed the significance of measurements of the dependence of sensory thresholds upon the rate of application of the stimulus. The present paper describes measurements of this kind for cutaneous touch and pain, the sensations being produced by the application of a needle point to the skin of the finger.

It is usual, in measurements of thresholds for cutaneous touch and pain, to express the intensity of the stimulus in terms of the force applied to the skin, or in terms of the force and some function of the dimensions of the stimulating point (cf. Nafe, 1934), and it was at first intended to do this in the present experiments. An apparatus was therefore constructed in which a small sewing needle could be applied to the skin, the force on the needle being varied by varying the tension on a very fine rubber band. The apparatus was similar, in principle, to that described in the writer's previous paper. A small galvanometer mirror was attached to the needle, and a photographic record of the movement of the needle was taken by throwing a spot of light from the mirror upon a moving film. It was found that when the force acting on the needle was suddenly increased the needle did not merely move rapidly downwards into the skin, but, owing perhaps to the elasticity of the skin, executed a series of vibrations. (With the actual needle employed these vibrations were at a frequency of about 100–150 per sec., and there might be four or five vibrations comparable in magnitude to the original deflexion of the needle.) Theory indicated that to avoid this, while maintaining the other essentials of the method, it would be necessary to reduce the inertia of the needle more than seemed practicable. An alternative, of course, would have been to use a small elastic body, such as a Von Frey hair, instead of a rigid needle, and to vary the rate at which this was applied to the skin. But the examination of the movement of an apparatus of this kind presents serious difficulties.

It was decided, therefore (at the suggestion of Mr A. F. Rawdon Smith), to abandon the principle of applying a measured force to the skin, and to adopt instead that of applying a measured movement to the stimulating point, and hence a measured deformation to the skin, the point at the beginning of each reading having rested very lightly against the skin. In a subsidiary experiment the elasticity of the skin was roughly measured and it was therefore possible to obtain approximate values for the thresholds in terms of applied force.

II. Apparatus

The point applied to the skin was that of a fine sewing needle. The needle was 0·3 mm. in diameter tapering in about 0·5 mm. to a point which was roughly spherical and of about 0·03 mm. in diameter. This needle *N* (Fig. 1) was attached to one end of a thin aluminium lever *A*, pivoted on point bearings at *P*. The lever *A* was 20 cm. long and weighed 7 g., the distance *PN* being 2 cm. and the distance *PC* being 14 cm. The corner *C* of the lever was held firmly against the under-surface of a bevelled glass plate *G*, by means of a tightly stretched rubber band *D*. The plate *G* projected from the periphery of a horizontal disc (not shown in Fig. 1) which could be rotated at various speeds (by means of a motor and suitable systems of gearing and pulleys) about a vertical axis so that the plate *G* would move in the direction of the arrow. Care was taken to ensure that the part of the plate touching *C* always moved exactly in the direction of the length of the lever *A*, so that there was no tendency for the friction between *C* and *G* to introduce any lateral movement or bending of the lever. When the bevelled edge of *G* passed over *C*, *C* would, of course, commence to move upwards at a uniform rate, causing a corresponding, but much slower, downward movement of the needle *N*. This movement would continue until the end of the lever *A* reached the adjustable stop *S*.

A galvanometer mirror *M* attached to the lever *A* enabled the experimenter to read the extent of the movement with accuracy during experiments. It also enabled photographic records to be taken of the movement of *A*; and examination of these records showed that even at the highest speeds the movement was free from perceptible vibration.

Using an apparatus of this kind it is necessary that the subject's skin should be held extremely still during readings. The subject's finger (index finger of the left hand) was therefore placed under a sheet of brass *B*, having a hole 4 mm. in diameter directly below the needle. Some sponge rubber *R*, under the finger, held the finger in such a position that an area of skin on the back of the finger and a centimetre or two from the base of the finger nail was exposed through the hole. The exact position of the hole on the finger was determined by the subject to suit his own comfort. The brass plate was attached to a platform *E* which could be raised or lowered by means of levelling screws, and the needle point and the skin below could be viewed by the subject through a microscope (×30).

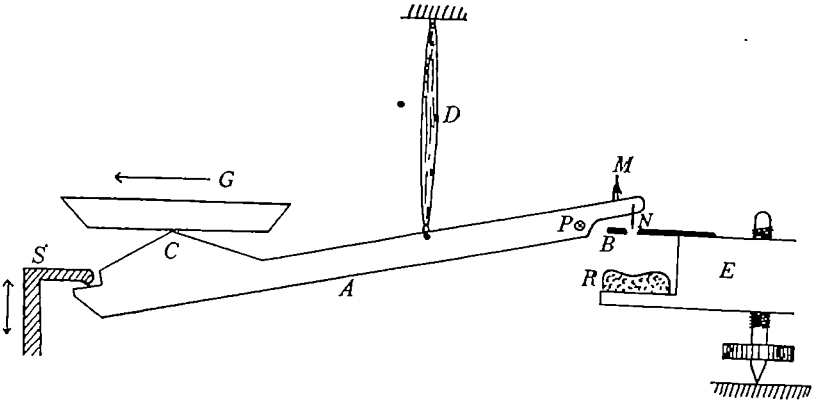

Fig. 1

III. EXPERIMENTAL PROCEDURE

The subject placed his finger in position, and an ink mark was made on part of the skin exposed by the hole to ensure that all readings were taken at exactly the same place on the skin. The microscope was focused on the extreme point of the needle when the needle was in its zero position (i.e. with *C* resting against the underside of *G*) and the subject then raised the platform *E* very gradually until the skin just touched the needle point. This adjustment (which was checked before each reading) could be made with great accuracy with the aid of the microscope.

Measurements of the thresholds for touch and pain were then made at each of four speeds of movement of the needle point (namely 1 mm. in 0·14, 1·1, 2·5 and 5·0 sec.), the method of taking the readings being similar to that described in the previous paper. The subject was allowed to remove his finger from the apparatus for a rest pause whenever he wished to do so, microscopic observation of the ink mark ensuring that he afterwards returned it to the same position. At the end of each series of readings the original reading was repeated to ensure that there had been no large general change in sensitivity to touch or pain during the series.

IV. RESULTS

Six curves were obtained in this way for touch and pain. In all of them the touch threshold showed a marked increase when the movement of the needle was made slower (the threshold at the slowest speed being from three to six times that at the highest speed), whereas with pain the threshold showed no significant change. In Figs. 2 and 3, which refer to touch and pain respectively, the broken curves are typical individual curves and the continuous curves represent the average of all six individual curves.

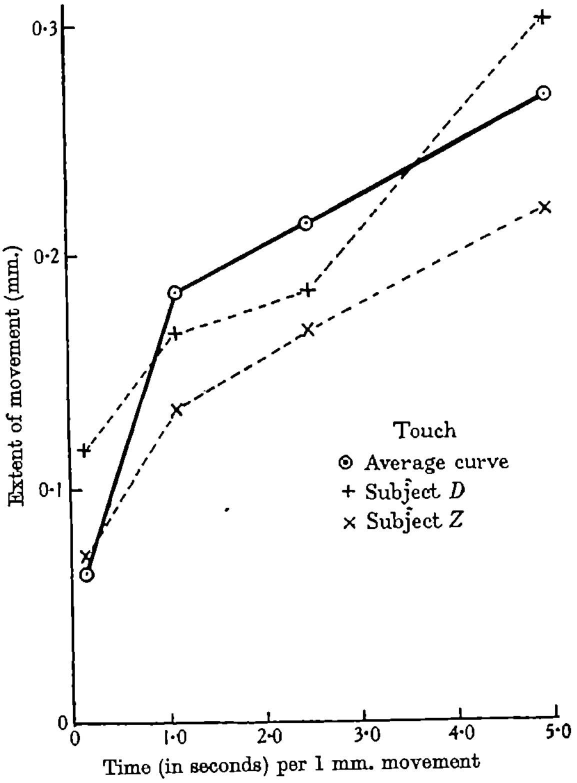

Fig. 2. Touch

When the movement produced a sensation of touch the subjects reported that the sensation occurred immediately upon the application of the stimulus, but when the movement produced an experience of pain as well as of touch they reported that there was usually a delay (sometimes of the order of 1 sec.) between the experience of touch and that of pain, even at the highest speeds. This delay, which appeared to be very variable, was not measured in these experiments. In a few cases the subjects described their experience as "tickle" which could not be identified with touch or pain. These cases were too rare for quantitative treatment; they were ignored in determining the pain threshold.

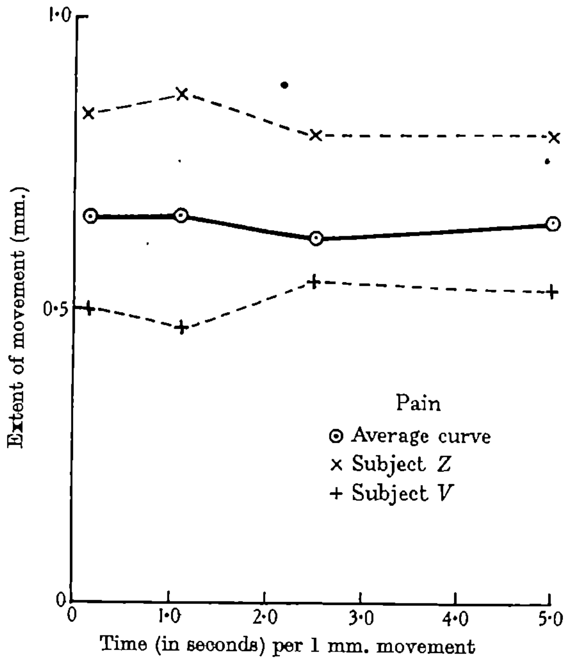

Fig. 3. Pain

V. DISCUSSION

It was pointed out in the introduction to this paper that in some preliminary work in which a known force was suddenly applied to a needle, the point of which rested against the skin, the damping of the system was insufficient to prevent the needle from executing a series of vibrations. The effect of vibratory motion of this kind may well be greater than that of a single excursion of similar amplitude, owing to the possibility of peripheral or central summation. This form of apparatus was therefore not used in these experiments, but it is worth pointing out that a similar effect is likely to occur in many forms of aesthesiometer.

In the form of apparatus used in the main experiment the stimulating point did not vibrate appreciably, but there is, of course, the possibility that a sudden movement of the needle may have thrown the skin into vibration. No method of testing this was available. If it is true that the skin vibrates appreciably under these conditions, and if peripheral or central summation occurs, this may be, in part at least, the explanation of the lower threshold for rapidly applied touch than for slowly applied touch. If, however, the vibration of the skin is unimportant, the explanation probably lies in the rapid adaptation of the end-organs concerned, as has been suggested in the previous paper. This latter explanation is consistent also with the constancy of the pain threshold at different speeds, since pain end-organs adapt relatively slowly (Adrian, 1932). There is also a possibility that the constancy of the pain threshold may be causally related to the long latent period of pain sensations.

The thresholds measured in this experiment have been expressed in terms of the extent of the movement of the needle point. In a rough subsidiary experiment some measurements were made of the force required to produce equal movements of the needle point under these conditions. The average of several measurements showed that the force required was of the order of 0·3 g. weight per 0·1 mm. movement of the needle, but there was, of course, a wide variation from subject to subject and from place to place on the finger. These measurements give average values of about 0·3 g. for the touch threshold at the fastest speed, and about 1·5 g. for the pain threshold.

VI. SUMMARY

Thresholds have been measured for cutaneous touch and pain, the sensations being produced by applying a needle point at various speeds to the skin of the back of a finger. The thresholds are expressed in terms of the extent of the movement of the needle point, i.e. in terms of the deformation of the skin. It was found that for touch the threshold varies markedly with the rate of application of the stimulus, being higher for slow speeds; but that for pain the threshold is independent of the rate of application over the range which has been studied. It is suggested that these results are most probably explicable in terms of the different rates of adaptation of touch and pain receptors; but other possible factors tending to produce the same result are pointed out.

I wish to express my thanks to Prof. F. C. Bartlett for his interest and advice.

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THE MEANING OF *G* AND OTHER FACTORS IN TERMS OF RATES OF OUTPUT

By S. J. F. PHILPOTT

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I. *The problem* (pp. 196–199).
II. *Problems of correlation and interpretation* (pp. 199–206).
III. *The o factor* (pp. 206–210).
IV. *The p factor* (pp. 210–216).
V. *The curve of output* (pp. 216–221).
VI. *Fatigue measures* (p. 222).
VII. *Summary* (p. 223).
References (p. 224).

I. THE PROBLEM

THE conceptions here put forward constitute an attempt at symbolic expression. To the extent that our definitions are at fault, the analysis fails. Although illustrations will be mainly from correlational studies, my own work has been in an entirely different field, that of fluctuations of attention. Studying the way people fluctuate, plotting their rates of output per second or other convenient time interval, one comes to think in terms of output rates. Thus, for example, the mark obtained by subject X in a given test can be expressed as

$$M_x = R'_x T,$$

where R'_x is the rate at which he works per unit of time, and T the total time taken. Thinking still in terms of output, the said mark M_x represents not merely a number, but an area. As symbolized in Fig. 1, it is the area enclosed by perpendiculars dropped from either end of the output curve to the base-line.

The two-factor theory divides M_x into two parts depending on the amounts or proportions of g and s present. Symbolically,

$$M_x = a.g + b.s.$$

On the other hand, it has proved impossible to explain fluctuations of attention on a two-factor basis. So that although the *area* of the curve in Fig. 1 can be attributed to general and specific elements, the fluctuations at its upper edge have to be otherwise explained. The hypothesis I have elsewhere put forward (1, 2) is that on any given occasion the subject produces, as it were, a random handful of the waves possible to him, and that whatever the nature of the task. The mechanisms determining the single waves are thus of general purposes utility, going into action, or coming out, in apparently random fashion. Yet one cannot but feel there should be some connexion between the elements determining output and fluctuations respectively. Our present task therefore is to attempt an analysis deriving general and specific elements from others of general purposes utility. In so doing, we shall actually be trying to resolve one of the main problems of the Spearman-Thomson controversy, although the correlational side of that matter will not be discussed (3, 4, 5, 6).

The picture Spearman gives (3) is of a mental factory in which energy from a (general) power station is supplied to various (specific) machines.

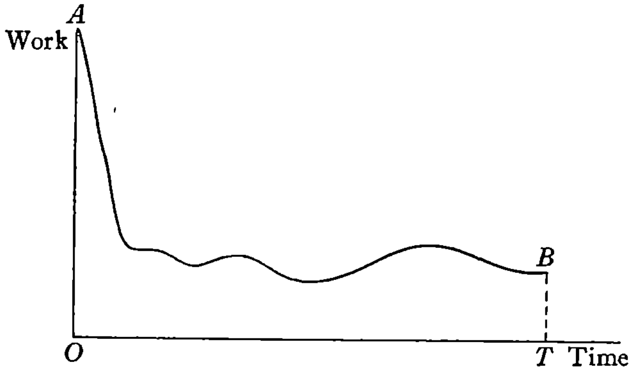

Fig. 1. The area referred to in the text is *AOTB*.

Output from any one machine is thus determined by the goodness of the central engine and of the machine concerned. Since fluctuations are set up by elements of general purposes utility, they cannot derive from engine or machines. We could conceivably bring them in by postulating a third set of elements, namely a fairly large number of workmen. If they work in fluctuating fashion, one stopping to rest after every second, another after every 2 sec., and so on, each man with his own period, we might obviously get a complex wave system in the final output curve. If in addition we suppose the men to be available for use with any of the machines and that they go away to their common-room whenever they feel inclined, we arrive at a fairly complete picture.

Although fairly complete, the picture is lacking in at least one important respect. It is an observed fact that if work is interrupted in such fashion that new waves are engendered, output goes up, thereafter falling again, as though with new waves there comes a tapping of new energy sources.[1] It follows almost certainly that we must associate waves with energy supply. The central engine of the Spearman picture must go, being replaced by a large number of separate energy sources, each perhaps with characteristic wave, and all of general purposes utility. Assuming that, the workmen also may go, for they were only needed to introduce fluctuations. We are left with many energy sources, feeding specific machines.[2]

Dropping figurative conceptions, let it be granted that the subject can call at random on many sources of output. Let rates vary slightly according to the source tapped, but let mean rates for subjects X, Y, Z be R'_x, R'_y and R'_z respectively, there being individual differences. For purposes of any given performance, let every subject take N sources into use, producing what may be called N 'threads' of output.[3] Then apart from an error of sampling, the mean rate of output for subject X will be N times his mean rate per thread, the total amount in time T being

$$M_x = NR'_xT. \qquad \ldots\ldots(1)$$

The symbol R' represents a mean rate, varying from person to person. Let there be a unit rate, R, so that R'_x becomes k_xR. Whence

$$M_x = k_xNRT. \qquad \ldots\ldots(2)$$

We here introduce a *general* element in that people vary in their k coefficients. But there are no specific elements in the correlational sense. Specificity of that kind is approached when allowance is made for special aptitudes, and changes in rate of output dependent thereon. Thus, for example, if the task be addition, and if subjects X and Y have equivalent basal $kNRT$ areas, they will nevertheless vary in output if X has learned to add by double columns, while Y still works by single columns, adding first the units, then the tens, and so on. In such case, X will clearly excel over Y, although the tables might well be turned on him in other performances. Symbolizing

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[1] Let a group of subjects be told to begin work on a given task, and to change, at a given signal—without stopping—to another kind of work. Certain waves will run right through the double or sandwiched task. There will also be a decrement running throughout. The curve will nevertheless rise sharply at the point of change, thereafter falling off, showing that new and 'unfatigued' elements have entered into the complex ((1), Part II B).

[2] The notion of 'machines' in a factory may be misleading. It implies the making of stereotyped products. It is better in many ways to regard the specific element as something more in the nature of a complex system of levers, etc., through which energy is transmitted, just as in the body, the bones, muscles, and nerves are employed, whether in gardening, cycling, racing, or what not.

[3] N is here taken to be constant for all people in all circumstances. It need not be, but it hardly matters. If it varies at random, then the differences will be taken up in the error of sampling. If it varies generally, some subjects always tending to use more threads than others, differences will be taken up in corresponding k coefficients. If it varies specifically differences will be taken up in a coefficients, and so on.

$$M_x = (ak)_x NRT, \qquad \ldots\ldots(3)$$

where the a coefficient represents the extent to which X increases his rate in respect of specific aptitude (innate or acquired) for the task concerned.

If the formula were left as it stands, a would have to cover things specific to the way the task is assessed. Thus, for example, if the scheme of marking is altered in such a way as to double all the marks, every subject's a will be doubled. It is therefore better to indicate specific elements of the kind by a constant symbol, so that we get

$$M_x = (ak)_x SNRT, \qquad \ldots\ldots(4)$$

where S is common to all the subjects concerned.

II. PROBLEMS OF CORRELATION AND INTERPRETATION

At this point it will be well to turn to problems of correlation, for we need to break the a coefficient down a little more to cover matters of g saturation and the coming of group factors. Put generally, our concern for the moment has been with *absolute* amounts of work done. For correlational purposes, the only things that matter are *relative* proportions. That means we can at once eliminate S, N and T, for they are common to all the subjects concerned. They will not affect correlations. Dividing them out, there remains

$$\text{Rate}_x = (ak)_x R, \qquad \ldots\ldots(5)$$

this being the average rate of output per thread for subject X, freed from the idiosyncrasy of the system of marking, etc., the symbol R being left because it represents a unit.[1]

What now of g saturation? By definition, a test of high g content calls for a high proportion of g as compared with s. In our present terminology, k must be large as compared with a. But k is constant for the individual in all circumstances. To arrive at variations in relative weight, we must therefore vary the absolute amount or value of a alone.[1] Take the extreme case where two tests have so high a g content that the correlation between them approaches $r = 1{\cdot}00$. Such a result may be expressed by varying a in one of three ways. It may (i) be made to approach unity for each of the performances, or (ii) it may be left of appreciable size as long as subjects are self-consistent, tending to give the same a in each of the two tests concerned, or (iii) still being of appreciable size it may tend to be of the same (i.e. constant) size for all the subjects concerned, whether in the one test or the other.

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[1] The formula covers goodness as well as speed, for output is in terms of items of given standard of goodness per unit of time. It is possible to set tasks in which goodness alone counts, there being no time limit. Such cases are ignored for present purposes. Our immediate object is to get formulae that can be used in discussing not only problems of g, but also o and p factors, the tests for which are essentially speed determined.

Suggestion (i) is a limiting case of (iii), but is given separately because of its significance. Making a equal to unity means its virtual elimination. Rate of output will depend upon k alone. But this being constant for the individual it follows that rise in g saturation would mean closer and closer approximation to a fixed and *minimal* speed level. Such a conclusion is not likely to be true, speed and g content tending to go together.[2] There remain possibilities (ii) and (iii). Both assume a common element, either subject or not subject to individual differences respectively. Whence, putting appropriate coefficients inside and outside the brackets, we get

$$\text{Rate}_x = (abk)_x R, \qquad \ldots\ldots(6)$$

or

$$c\,(ak)_x R, \qquad \ldots\ldots(7)$$

where the a coefficient now represents residual and uncorrelated values in respect of which the subject still differs specifically from his neighbours, and from his own performances in every other task.

For correlational purposes, this residual a coefficient is narrowly specific. The b coefficient is broadly specific, being common to at least two tests. It represents a group factor, an element causing tests to correlate more highly than they would by reason of k alone. c, being common to a group of subjects (in respect of at least two tests), will not affect relative orders of merit in those tests. Correlation will in such case depend upon k alone. It is in this last case therefore that we find an answer to a problem mentioned a moment ago, namely that of g saturation. It has to be interpreted in terms of variations in a, that being usually effected by taking out a common element c.

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[1] For all practical purposes, a correlation coefficient measures degree of relationship between two orders of merit. If a group of subjects has been ranked for two different performances, those orders of merit will not be changed if we multiply or divide all the marks by a constant amount. For that reason S, N and T have been eliminated. Or again, if two performances depend upon k and a, and if the two orders of merit are the same, then k being constant, a must also have been constant. But by definition it is not supposed to be constant. It should vary from test to test. And according to the strength of a in those various tests, so the correlation will rise and fall.

[2] It is assumed that a is normally greater than unity. If it is less, then its elimination will naturally mean an increase in speed towards the uniform k level. The general notion here put forward can be illustrated by reference to tests of association. They can be free or constrained. The degree of constraint may be slight (as when answers are confined merely to a class) or so extreme that only one answer is appropriate, as, for example, in the test of opposites. On the whole, the greater the degree of constraint, the greater the g content, and the shorter the reaction time.

Having thus defined a, b, c and k, it is necessary to look at their possible meanings in terms of psychological processes. The simplest plan will be to keep within an already existing set of definitions, those of the Spearman scheme. To the extent that k is assumed to be constant for the individual, there is conformity with the Spearman law of span, namely, that the mind tends to keep its simultaneous output constant. It should, however, be noted that the assumption was not made merely for the sake of conforming to that law. It is a necessary condition of the analysis. Two apparently contradictory sets of experimental findings have to be reconciled, namely, that output seems to be determined by general, group and specific factors (as studied by factor analysis), whereas output curves themselves can only be explained in terms of threads occurring at random. Reconciliation is effected by assuming output on any given occasion to be determined by a bundle of threads of mean rate kR, which gives a general tendency to the results. There is difficulty in that g, the general factor, may be traced in performances depending on goodness of response alone, no account being taken of time. On the other hand, this same difficulty of speed and goodness attends the interpretation of correlational data, and as it is better dealt with on that side, we may perhaps leave the matter by quoting an appropriate statement by Spearman:

> On the whole then, g has shown itself to measure a factor both in goodness and speed of cognitive process. Such goodness is here taken as at bottom indicating clearness.
>
> The connexion between the goodness and the speed is that of being interchangeable. If the conditions of the case are such as to eliminate the influence of speed, then g measures goodness, and vice versa. ((3), p. 258.)

Turning to the problem of R, the unit of rate, quotation may again be made, this time of the Spearman law of clearness:

> ...All knowing develops by way of increase in clearness and speed; that is to say, every item in the cognitive field, every constituent in whatever object (mental) is perceived or thought of, comes into being by a continuous emergence out of utter obscurity up to some degree of clearness; and as this description implies, the emerging occupies some duration of time.... ((3), p. 244.)

There is here the essential element of mental life. Fundaments, whether experiences, relations or correlates, come and go. It would therefore seem appropriate to define R in terms of the moving of a unitary fundament through unit clearness in unit time. The analogy is obviously with physical horse-power as measured in gram-centimetre/seconds, although as obviously it remains an analogy. Physical energy may be at bottom responsible, but we have no present means of relating it exactly and precisely with things mental. Mention of the analogy does, however, bring to mind some of the difficulties of the conception. Physical weights and distances can be measured. On the mental side we cannot measure either the "mass" of a fundament or a "distance" on the scale of clearness. All that can be done is to assume equivalence as between successive items in a task. The assumption of course made in practice, otherwise measures in terms of sums done, or letters cancelled, or analogies solved, would be meaningless. And it is perhaps worthy of remark that the most successful mental measures are precisely those in which equivalence of the kind is most likely to obtain.

The definition of R as a rate of coming to clearness gives us two problems, namely, as to the kind of mental objects that will come, and as to the switching mechanism whereby they are brought in and out of the focus. Figuratively speaking, it is as though we measure output in terms of counts made by an automatic turnstile. If the turnstile is of general purposes utility, differences in speed may occur with people of different sexes, weights, and so on, but in a very real sense the differences will lie with the people, not with the turnstile. The point is made because although Spearman's two quantitative laws of clearness and span have been adopted, there is difficulty with his qualitative analysis. He draws a distinction between processes of eduction and reproduction, identifying g with eduction alone.[1] Two possibilities at least lie before us. We may postulate two kinds of objects, educts and reproducts, both using the same stile, i.e. both coming to clearness, switching in and out of the focus. Or alternatively, we may virtually abolish the distinction, saying there is only one process (redefining "eduction" if need be) and making the element of reproduction something analogous with, say, weight in the fundaments, a characteristic rendering it more or less easy for them to slip through the stile. For whatever we suggest, it still remains that k is a coefficient of rate, a measure of speed with which objects pass the stile. If we assume educts and reproducts as essentially different mental objects, and if there are differences in the rates at which they pass, then such differences must be expressed under a, b or c, and most certainly not under k. In other words, if eduction and reproduction are both *processes*, and if g (or k) derives from or measures the energy used in implementing them as processes, then both are measurable on the specific side, presumably as group or as common elements or factors. To make eduction fundamental in the sense that it is 'co-extensive with' or 'identified with' g, we must assume that all processes are at bottom eductive, and that reproduction is not so much a process as a facilitating condition. In fact, it would be better to drop the name as misleading, speaking rather of facilitation.[1]

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[1] See, for example, *The Abilities of Man*, where the three noegenetic processes are said to "make up absolutely all the cognition (other than purely reproductive) of which the human mind is in any circumstances capable" ((3), p. 167). Or again "...in any genuine eduction, the required response is intrinsically determined by what has been given to start with, so that whatever constituent in the response has *not* been thus determined must needs be credited to reproduction" ((3), p. 273). Or see *The Psychologies of* 1930: "...g has shown itself to be coextensive with the two noegenetic processes called the eduction of relations and that of correlates...as a general fundamental fact—making due reservation for all the inevitable additions and corrections to befall eventually—this identifying of g with eduction promises to serve as a polar star to guide our further advances throughout the region of individual psychology" (4).

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[1] The whole problem is of 'picture making'. We can suggest any scheme we like as long as it is consistent. Take, for example, the Spearman processes of clearness variation, disparition and recall. We here say that things may vary between the two extremes of obscurity and full clearness, while still remaining above the level of awareness. They may then go completely out (disparition); they may return (recall). But where is the evidence for disparition? Suppose the curve of clearness variation is asymptotic? In such case, disparition as a process would go, and with it recall, leaving but the one process of clearness variation.

The notion is quite understandable. All the things of which I have ever been aware will remain in mind, but most of them at the lowest possible limit of clearness. They will constitute a functioning whole, influencing my future awareness, determining my 'attitudes' toward them, and so on. We thus also allow for the underground workings called for by the Freudian and kindred schools. There is here the chief objection to defining R in terms of clearness, the latter seeming so characteristic of the focus. If a problem is solved in sleep, through how many degrees of clearness is it shifted in the process? Is a different kind of determinateness involved, or is it a case of low intensity working, with compensatory change in time value? Any adequate picture of mental 'horse-power' must ultimately cover (or reject) several antithetically expressed processes of the kind, eduction and reproduction, conscious and unconscious working, and so on. Difficulties of the kind are not confined to any one system. One might quote from the Gestalt school, to show that troubles arise as soon as automatic (associative) reproduction is admitted as an independent process. It is the age-old problem of new wine in old (conceptual) skins, or the equally ancient difficulty of expressing many-facetted reality in terms of one facet. The new quantitative facts are not in dispute. Thus for example, g (or k) measures a general tendency. Trouble comes when attempts are made to clothe that tendency in older qualitative terms. Or again, R is a rate of output. To say it is rate of coming to clearness may be to go beyond the data.

Turning to another aspect of the problem of eduction and g, note should be made of

With these problems in mind it will be of interest to look at the examples used by Spearman to illustrate the eductive-reproductive problem, namely, the following analogies:

(i) SNOW : WHITE :: COAL : ?

(ii) WHITE : SNOW :: BLACK : ?

(iii) BLACKSMITH : HORSESHOE :: BUILDER : (design, survey, *dwelling*, painting).

(iv) DUET : ORCHESTRA-PLAYING :: PARTNERS : (*whist*, company, business, finance).

Of these he says:

> (i) ...the ideas given to start with (snow, white and coal) completely determine the response 'black'. This response is wholly self-evident; it could be effected by a visitor from Mars (provided of course he has the starting ideas). ((3), p. 273–4.)
>
> (ii) ...the ideas given to start with here do not determine any response completely (beyond the bare idea of 'a-black-thing'). No detailed response is wholly self-evident; the visitor from Mars, however great his *g*, might well be baffled. The evoking of a correct response...requires therefore not only some eduction...but also some reproduction (to get to such a particular idea as 'ink').... ((3), p. 274.)
>
> ...in (iii) the operation is based throughout upon words that may fairly be assumed as well known to all the subjects under test, so that only the superimposed eductions are 'critical' in supplying a process that some subjects can accomplish but others not. In (iv), on the other hand, there is obviously needed some previous acquaintance with the game of whist, an experience far from being universal...."Eduction and not Reproduction is the only reliable basis of a successful test."... ((3), pp. 274–5.)

The comment on (ii) does not necessarily indicate why the question is bad. Each and every subject is called upon to apprehend the instructions, as taken in conjunction with the given questions. Having thus educed the kind of thing he is supposed to do, it is but a further eduction to arrive at the fact that the answer is only constrained in that it must

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the theorem of indifference of the indicator ((3), p. 197). It is that any test of *g* may be employed as long as it correlates highly enough. "...With this proviso, the most ridiculous 'stunts' will measure the self-same *g* as will the highest exploits of logic, or flights of imagination." In other words, we must not seek *g* in the qualitative nature of the processes employing it. But this is exactly and precisely what is done in identifying *g* with eduction. For, quoting again, "...the prime condition for the appearance of *g* in any ability might naturally be expected to lie in the quality of the cognitive processes which it involves...the plain answer...is that *g* occurs only when the abilities concerned...are noegenetic..." ((4), p. 348).

Apply this same criterion to other cases. There are bodily and physiological measures that correlate with *g*. Spearman himself refers to the work of Travis on the knee jerk as epoch-making ((4), p. 345). Suppose, therefore, that a physiological battery of tests were drawn up to give a *g* measure. Should we then naturally expect to find the qualitative nature of *g* in the nature of those processes? If not, why should the analogous argument be advanced in the case of noegenetic processes?

be some one object of a given class. It is not very apparent why it should be more troublesome to give one out of several equally possible answers. So that if eduction were the main process involved, there is no apparent need for a loss in g content.

A possible reason is given by introspection. For even although common sense ought to tell the subject to give the first answer coming to mind, there will nevertheless be side chains of eduction in search of best-fitting responses, the more able subjects possibly sinning more in this respect than others. Two points of present interest arise. The coefficient a varying at random is thus shown to depend (to some extent at any rate) on the number of ways in which subjects may differ in their methods of approach,[1] while in the second place, those side-chains of eduction use energy not accounted for in the experimental figures. Fundaments, relations and correlates occur, but are not represented. Whence, amending the "Eduction and not Reproduction..." dictum, we might get "Single-line working rather than Side Tracking is the essential basis...."

In a sense, this is derogatory to the intelligence of the subjects concerned. They have to be put in blinkers to make them walk in a straight line. But that of course is beside the point. It is the length of the line that we desire to measure. If, by side-tracking, they tangle it, then we must try to prevent them from doing so.

Cases (iii) and (iv) are slightly different. There is still the problem of constraint to a single path. For in solving (iv) one has to send out several side-chains to inquire whether two duettists make an orchestra in the same sense that two partners make a four at whist; whether an orchestra can be said to contain duettists at all in the sense that a whist four essentially consists of two pairs of partners, with even more remote explorations to find out whether the orchestra symbolizes merely a hand at whist, or a drive. But the case also goes over into the problem of necessary information. As Spearman points out, subjects must have the same amount of information on the subject-matter of the questions, otherwise the test is not of g but of attainments. And in that respect, a, b and c coefficients are of course involved.[2]

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[1] Cf. Spearman((3), p. 241) "...if only a test could be so fashioned as to eliminate all possible differences in the subjects' manner of procedure, then this single test might by itself conceivably afford a perfect measure of g."

[2] We here get close to the verbal group factor. If all people in a group have equal facility with the words used, that element comes under c, a common coefficient, and correlations are unaffected. But if some are more facile than others, they will gravitate to the top of the class list in respect of the given tests, and to an extent determined by v as well as g.

Coming back to the general considerations with which we started, the search for constancy in any speed test of g is obvious. In the test of opposites, for example, we choose only well-known words; we choose a simple relation, that of opposition, and so on. Throughout, there is the search for constraint in respect of possible answers, and constancy of knowledge. But it should also be noted that this may well mean constancy in the eductive element. We use simple and well-known relations. In effect, we are hoping the subjects will find them of common difficulty. So that what may be called 'eductive value' can come under c as well as 'informational value' and by the same argument. For if one becomes constant, informational items being of the same weight for the various subjects concerned (although not necessarily equal to one another) why should we assume that the other remains variable (cf. argument under (iii) above)? Is there any reason save the belief that eduction and g are one and the same thing, so that 'eductive value' *ought* to remain variable?

Summing up, rates of output have been expressed by the coefficients a, b, c and k. Of these, a, b and k correspond with specific, group and general factors in the Spearman scheme. The unusual measure is c, which covers elements of common or constant weight for all subjects in the group tested. kR being constant for the individual, variations in g saturation have been explained in terms of c content. As that common element rises and falls, so also does g. If eduction and reproduction are to be regarded as independent processes, we must place them co-equally under b and/or c, making them group and/or common elements. If eduction is to be taken as the process or power measured by g or k, the reproductive element must be regarded, not as a process, but as a facilitating condition.

III. THE *o* FACTOR

According to the Spearman system, the oscillation factor o is a measure of the oscillation of g. It is measured in terms of amplitudes, and the question at once arises how they may be expressed.

The basal formula $M = NRT$ represents an area freed from individual idiosyncrasies of the subject and the task. It stands for something potential, something waiting to be clothed in, or modified by, coefficients appropriate to the circumstances of the case. So that if A is taken to represent the mean amplitude of a bundle of N threads of (potential) output, and if we multiply by $(ak)_x$ to clothe those threads, it follows that A is increased by those same coefficients. Whence

$$\mathrm{Amp}_x = (ak)_x A, \qquad \ldots\ldots(8)$$

this expressing the actual or apparent amplitude of the waves at the upper edge of the area; or it would do, were there no influences affecting amplitudes as such. There may be such elements, some general, some specific. (There is a complication, for example, in that the sum of the amplitudes of N waves is not the same thing as N times the mean of their various amplitudes taken separately.) All such disturbances can be absorbed into two extra coefficients a' and k', making the expression run

$$\mathrm{Amp}_x = (a'k')_x \, (ak)_x A. \qquad \ldots\ldots(9)$$

We ought also to take account of b and c coefficients. It will, however, suffice to take equation (9) as characteristic.

Flugel(7) and his students have found a positive correlation of fair size between area and amplitude measures in the same performance. This follows, for they are comparing

$$\mathrm{Rate} = (ak)\, R \quad \text{with} \quad \mathrm{Amp} = (ak)\, (a'k')\, A,$$

and correlation can occur through the common elements a and k. There has also been found a positive correlation, although smaller, between amplitude in a given performance and scores in a standard g test. Again this would follow, correlation coming through k.

A difficulty arises with relative amplitudes. They are obtained by dividing the above measure of (absolute) amplitude by output in the performance concerned, this giving

$$\mathrm{Rel.\ Amp}_x = \frac{(ak)_x \, (a'k')_x A}{(ak)_x R} = (a'k')\, A/R, \qquad \ldots\ldots(10)$$

where A/R, being a ratio between units, may be ignored.

The general element here is k', and relative amplitudes from different performances will correlate in virtue thereof. From the work of the Flugel group already mentioned, we find that they also correlate negatively with g scores. So that the greater a subject's k, the smaller his k' and vice versa. While since correlations with output in the same performance are even more highly negative, we must assume that the more specific aptitude the subject has for the said performance (as seen in his a coefficient) the smaller his a'.[1] There is here some ground for

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[1] In correlating relative amplitude (10) with output in the same task (5) we are dealing with a ratio. Suppose absolute amplitude is put equal to a constant C. The required correlation will be between

$$C/(ak)\, R \quad \text{and} \quad (ak)\, R/1.$$

Clearly, $r = -1{\cdot}00$. The high negative correlations met with in practice are thus evidence that

Flugel's contention that fluctuation is a sign of weakness, but only of course in the comparison between (ak) and ($a'k'$). In so far as amplitudes are determined by (ak) alone, they are of strength in the sense that those coefficients measure goodness of mental power.

In trying to attach a meaning to ($a'k'$) it may be said that the greater the specific ability a person has acquired or inherited, or the greater his general ability, the more he tends to maintain a constant rate of output, adapting himself perhaps by (unconsciously) refusing to speed up when crests in the attention wave occur. Or a better notion is perhaps that people of low ability oscillate more, relatively speaking, because they stop at every twist and turn in the material. For a big a coefficient may mean facility and experience of every conceivable difficulty, a small one bespeaking ignorance even of the existence of such difficulties, with consequent increase in a'.

A view having origin in some unpublished results of Ba Htay, and quoted by Flugel ((7), p. 32), is that if o measures the oscillation of g, there can be no specific correlation on the o side, amplitude measures thus having no 'reliability' save that coming through o alone, while even if we use a battery of tests known to be linked by a group factor on the ability side, there should be no signs of such a factor in respect of amplitudes.

I fail to see any logic in such a case.[1] It is true that using group-linked performances of the postulated kind, Ba Htay actually *did* arrive at low reliabilities and but weak indications of a group factor in their amplitudes. On the other hand, the findings are quite easily explained in terms of what has been said in the last few paragraphs. Using performances linked by a group factor, we need to extend the amplitude formula, making it

$$\text{Amp}_x = (a'b'k')_x \, (abk)_x \, A. \qquad \text{......(11)}$$

Suppose now that ($a'b'k'$) and (abk) are inversely related, and that their product tends to be constant for all subjects. We shall at once be

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absolute amplitudes are tending to constancy. It is understandable, for there seems to be inverse relationship between (ak) and ($a'k'$). Suppose three subjects to have (ak) values 6, 3 and 1 respectively, with corresponding ($a'k'$) values 1, 2 and 6. There is inverse relationship. On multiplying the said coefficients (as in formula (9)), the products become 6, 6 and 6. The constancy attained in practice will not be as perfect, but there will be the tendency.

[1] It is difficult in any case to say just what is meant by the statement that o measures the oscillation of g, unless of course we side step from o and g as *factors* to the presumed underlying mental energy. But even admitting it, why should it mean absence of specific correlation on the one side more than on the other?

able to extract quite a large c coefficient from the right-hand side of equation (11). But by definition this means diminution of corresponding a and b values, with loss of group-factor tendencies. It is true that when discussing the ability side, it was said that the coming of c means a rise in the relative importance of k. Here, however, in the case of amplitudes, we are making a c coefficient at the expense of k as well as of a and b. Individual differences will thus be correspondingly lowered, and we at once explain low reliabilities, for orders of merit depending on small individual differences are notoriously unsteady.[1]

Amplitude measures are easily and directly made in what are commonly called work tests, i.e. supraliminal performances such as dotting or computation. They are more difficult to make in work at the threshold, in rivalry, reaction times, and so on. Tentative measures have been suggested. Thus, for example, with reaction times, it has been assumed that people of high o will do relatively badly if stimuli are given without a warning signal, the difference between average results with and without signal being taken as a measure of their oscillation. The same method has been adopted with responses to stimuli at the sensory threshold. But as Flugel reports (7) there seems to be no o in the results. There is no apparent reason why it should be present. Such measures are determined as much by period and phase as by amplitude, the results presumably varying with the position of trough points in the attention wave relative to the moment at which stimuli are given.[2] More interesting results were obtained by Chen (10) who measured rise and fall of the threshold by increasing and decreasing the stimulus value, taking note of points in the (stimulus) curve at which subjects reported emergence or disappearance of the stimulus. He thus arrived at a measure of range between appearance and disappearance values, finding that such measures—from different sensory thresholds—tend to correlate, as indeed they should, for a range measure of the kind depends largely upon amplitudes as we have defined them.[1]

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[1] Having been one of Ba Htay's subjects, I should say that his low reliabilities are partly due to difficulties occurring at random in the test material. I am not criticizing the tests as such; some were ingenious. But, for example, in cancelling circles (MacDougall's dotting test), not in conventional fashion, but in patterns or sequences, I found it frequently necessary to stop to refocus the pattern I was supposed to be employing. Anticipating what is to be said in a later section (p. 218), such stoppages and refocusings are likely to see the starting of new threads, with resulting complication in the wave system. (Amplitudes are negligible at the beginning of a new thread, thereafter opening out.) So that tasks characterized by stoppages will give apparent amplitudes determined by (*a*) big drops at points of refocusing, and (*b*) true waves between stops, of rather smaller amplitude than usual. In other words, the curve will be more of what I shall call type B than type A.

[2] Work by D. K. Grewal (8) on time judgement shows that they tend to be poor if and when the length of the interval brings the subject to a trough in the attention wave. It is conceivable that a person of low amplitude swing will fall into error less often than one of high amplitude, but it remains to be proved. In any case, periods being of the same general system for all people (subject only to the random sampling of threads already referred to) there will be less chance for individual differences to manifest themselves. Results similar to those of Miss Grewal, this time with delayed reaction times, are reported by N. Wallace (9).

IV. THE p FACTOR

Measures of p (perseveration or inertia) afford some curious problems, and test the present analysis somewhat severely. Defined in terms of the tests usually applied (the only useful definition for the purpose) inertia shows in a loss of output occasioned by a change in the method of performing a task. Thus, for example, the subject may be told to write the letter *e* as quickly as possible for a given time (thus establishing a norm), thereafter writing it backward (from tail to loop). Loss in output in the disturbed or Y operation is then expressed as a difference or a ratio, comparison being with the normal or X operation. Difficulties will arise with regard to b and c coefficients. For the moment, they can be evaded by bringing those coefficients under their corresponding a values. That done, and symbolizing rates in the two performances by X and Y, we get typical expressions as follows:

$$(X-Y)_x=(ak)_xR-(a^*k)_xR=[(a-a^*)\ k]_xR=(dk)_xR, \qquad \ldots\ldots(12)$$

$$(X/Y)_x=(ak)_xR/(a^*k)_xR=(a/a^*)_x=(d/a^*+1)_x, \qquad \ldots\ldots(13)$$

where the a^* values belong to the Y performance (k, of course, being constant whether on one side or the other), the final substitutions being obtained by putting $d=(a-a^*)$ and $a=(d+a^*)$ in the two formulae respectively.

The problem of p as a general factor is not too obvious. It must, of course, be contained within the difference d. For p to be general, d must tend to be constant for the individual, people of high or low p giving large or small values of d in all circumstances, there being, however, variations from test to test according to the strength of the specific (inertial) factors concerned.[1] In practice, measures, whether of the difference (12) or the ratio (13), behave as though there is such a general factor. To that extent, d is shown to be constant for the individual. There is also a correlation with g, the tendencies being positive and negative with (12) and (13) respectively. This may at once be covered by postulating negative correlation between g and d, people of high g giving lower values of d than they would otherwise do, and vice versa. So that on correlating measures of form $(dk)_x R$ with the results of a g test there will be positive correlation through the k values on either side, and negative correlation through d and k, the two tendencies neutralizing one another, or almost so, the balance being slightly positive. With measures of ratio (13), there being no k in the expression, we get only the negative tendency.

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[1] There is a subtle difference between Chen's amplitudes and those met with in work at, say, computation. In the latter case, we record rates of working at successive 5-sec. intervals, or other convenient period. The amplitude measure so obtained is thus a measure of variations in *rate* or speed. Chen's curve might be called that of rise and fall in sensory *ability*. We can relate the two by assuming an underlying curve of ability in computation, a curve made manifest in terms of variations in rate.

The point is of importance, for Chen's curves have periods belonging to the same system as those found in supraliminal work. So the waves are likely to be the same whether in one kind of work or in the other. Agreement in period so demonstrated now seems to be borne out in that there is agreement in respect of the amplitude factor.

The coefficients b and c were absorbed into corresponding a values for purposes of formulae (12) and (13). This was done because there are three sets to be considered. They are common elements, and it follows that there may be community between forms X and Y of the same nominal performance, between all the X operations as a group, and between all the Y operations. Taking these X/Y, X and Y coefficients into account would somewhat complicate the formulae. On the other hand, we need to inquire into their significance. It has already been suggested that rise in g saturation means rise in the size of c, the amount of group-factor saturation similarly depending on b. It follows that if, in the X/Y group, we make b and c grow at the expense of a, the difference d will tend more and more to disappear. In other words, high saturation with g (or a group factor) in both normal and disturbed forms of a test means relative absence of p.

An interesting speculation arises. Suppose we start with an X performance of high g saturation, i.e. one of large c content. And suppose we modify it slightly to get a Y operation of similar coefficients save that c has been slightly reduced by an amount d_1. Suppose further that successive levels $d_2, d_3, \ldots, d_n$ are possible, giving us variants $Y_2, Y_3, \ldots, Y_n$ of the same basal task X. Clearly, we are envisaging as many possible batteries of p tests as there are (relatively) constant levels of d, each battery presumably characterized by a group factor as well as by the central tendency p.

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[1] We ought to replace d by symbols representing general and specific elements respectively. There is no present need as long as its dual nature is recognized.

Curiously enough, there are difficulties in putting such a notion into practice. The normal or X group of tasks from which we should start is, by supposition, of high g saturation. As a matter of practice, conventional p tests are of moderate to low g, whether in respect of X or Y. So that whereas we can quite easily obtain a measure of d when c is of moderate size, we do not in practice do so when c is large, despite the fact that it might seem we have more elbow room for the purpose with that larger c. Or again, the fact that g has to be low indicates that the best p test material is such that a is large compared with b and c. So that pushing the present conceptions to the extreme case, it might seem that d is dependent on specific or a elements alone. This is an impossible suggestion, for by definition, a is specific to the behaviour of the given individual in given sharply defined circumstances. There is no overlap. Moreover, for any one person there is a random distribution of such elements, so that differences of the form d should vary at random as well. They do not. As a rises and falls in operations of the X group, so does a^* in corresponding Y operations. There is correlation, so that there must be b and c elements somewhere from which the constant difference d can be drained. If, therefore, it is an empirical fact that we cannot make an adequate or practically useful p test by disturbing a fully g (i.e. c) saturated performance, it is as clear a theoretical fact that we cannot go to the other extreme and use one that is completely unsaturated. There is obviously something measured by c (or by g) that is, as it were, inimical to p. It may be that p is entirely dependent on b elements. Alternatively, if c (or g) enters, there is non-linearity in the relationship, p being weak or absent from measures made at either end of the c (or g) range.[1]

The factor g has been placed after c as a possible alternative, merely to indicate that the manifestation might after all be traced to it. But, in fact, the most obvious suggestion is that the something responsible for c is the element in question. Or at any rate, one of the elements in c. Such an element might be facilitation. Interpreting, a necessary condition for a good p measure is some degree of facilitation in the X operation, otherwise there could be no loss on changing over to the Y form. As training proceeds, facilitation may remain unidirectional—there being no need or even positive disadvantage in upsetting that system—or it may tend to become multidirectional, associations working freely in all directions, as in the use of words and concepts in abstract thinking. If the latter, then clearly conventional p tests (with their dependence on upsetting a unidirectional tendency) become more and more difficult to devise.

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[1] It is not easy to illustrate, but at the high g end, let a list of opposites be drawn up in forward and reversed forms, as, for example, GOOD BLACK LARGE... and BAD WHITE SMALL.... There are hardly likely to be any very great differences in mean speed of working the two lists, so that d will tend to be zero. The interpretation to be offered in the text here most obviously applies, for associations tending to work as speedily in either direction (cf. footnote, p. 214).

Incidentally we thus arrive at a possible explanation of the negative correlation with g already mentioned, although at the expense of showing it to be more apparent than real. For let multidirectional facilitation depend upon b as well as c, there being individual differences. Let that b represent an element so broad as to be indistinguishable from g by correlational methods. In other words, let it be a constituent of g as normally measured. Then it follows that there must be a negative correlation with (apparent) g as so defined.

It is easier, however, to think in more concrete terms. Thus, for example, if a verbal form of education lends itself to a concentration on multidirectional training in the use of words and concepts rather than of things perceptual, then, as we pass along the list of all the possible performances in our search for p measures, the 'general' element in d will seem to drop abruptly as we cross from non-verbal into verbal material (using the terms here to indicate the presence or absence of v factor). Tetrads will be upset, and a group factor suggested. Moreover, people of high v will fall off in d more than people of low v. There will be negative correlation between p and v. To the extent that most g tests have a v flavour, (apparent) negative correlation between p and g is explained.

As a final suggestion in this matter of the correlation between p and g we may take the question of eduction. Deliberately to reverse an operation means cognizing the relation of opposition. If g is equated with eduction, then people of most g will be least troubled with that reversed operation—or so one might argue. There may be something in it, but the argument is two-edged. For take the test of opposites as such. If it is the relation of opposition that is in question, then the operation of cognizing a lot of different words in succession and educing their opposites ought to be *more* affected by p than that of cognizing the one element '*e* backward' some fifty or a hundred times in succession. Yet in fact, the opposites test is not likely to give a very good measure of p. The only obvious explanation is therefore that of uni- and multidirectional facilitation. It is not a matter of eduction as such.[1]

Two sets of group factors have just been under discussion, clustering about g and p respectively. The underlying suggestion is that they are functionally related. Mention should therefore be made of the fluency factor. Most of the earlier p tests (free and constrained association, ink-blots and the like) are now classed as f or fluency tests. By our present interpretation they presumably depend on multidirectional facilitation in the verbal field. In support of that conception is their high correlation with v, people of high v being of high f and vice versa.[2] Group factors of another kind were indicated some few paragraphs ago, when mention was made of several or many possible levels $d_1, d_2, d_3, \ldots, d_n$. Tests of reversed or of alternated symbols were in mind. They should be of two different levels, the difference giving rise to a group factor. The problem is of the number of such factors, and their use in practice. Would, for example, measures at two given levels measure the relative amounts of conative effort involved, and if so, how or why (cf. Stephenson (11))?

Before leaving the problem of inertia, reference ought to be made to the question of switching, especially as it leads directly to our next sections. The essential fact of mental life is the succession of items to and from the focus of attention. When an ambiguous drawing is set before a person, he presumably switches his mind towards it, and away from other lines of thought. He 'sets' himself towards the task of pressing a key as meanings in the drawing change, and so on. Call this the main 'set' or complex of 'sets'. If he attaches two meanings to the drawing—let it be that of the duck-rabbit—he will switch from one meaning to another in rhythmical succession, an important element in the case being the completeness of the change, and the quickness with which it seems to take place, once begun. Call these the major switchings. Within them will be minor switchings. While the meaning 'duck' is dominant, the mind turns now towards the eye of the animal, now the beak. Such minor switches are not recorded experimentally, but they exist. If now one of the major meanings becomes dominant, there will be no further switchings in respect of it. Thus, for example, if we decorate the drawing of a truncated pyramid, making it look like a carpeted 'corridor' with, say, windows at the side, and a door at the far end, the major meaning 'pyramid' need never come above the conscious level (save perhaps with a draughtsman of multidirectional facilitation in respect of lines of perspective). To all intents and purposes all switchings are of minor import, although it would be unsafe to say that no major movements take place at all.[1]

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[1] The trouble can be traced of course to the endeavour to identify eduction with g. For if we equate them, then negative correlation between p and g seems obviously as though it should be explained in eductive terms.

Incidentally we thus add another case to the argument of p. 206, where it was suggested that if the theorem of indifference of the indicator is ignored, there is nothing to stop anyone identifying g with things other than eduction. For, admitting that rise and fall in g saturation depends on rise and fall in multidirectional facilitation, we thus tacitly identify them with one another (*cf.* footnote, p. 212).

[2] It is rather a nuisance that the concept of inertia is in some ways a negative one. If a person is of high inertia, his d is large. But if our apparent inertia test proves to be of *fluency* rather than of inertia, then high fluency is associated with a small d. Hence the apparent change from what was said on p. 213 with regard to negative correlation between v and p.

Take now an inertia test, as, for example, that of alternated and reversed S. The subject has first to write the letter S as quickly as possible. The making of each successive letter may be regarded as equivalent to a minor switch as described above (always admitting, however, that the motor element of hand movement may complicate things, for in the case of an ambiguous drawing, there is of course no such imposed rhythm). The subject then has to do the letter in reverse, ƧƧƧ. By reason of the element of suggestion already given (whether in the instructions, or in the fact of doing SSSS in the *X* performance) the subject is now working with an 'alien' meaning in mind. There may be disturbances in the switching system. Finally, he has to do the symbol in its forward and backward forms alternately: SƧSƧ. He is here very definitely given a 'drawing' of ambiguous meaning. The problem of major switches arises. If merely left to himself to perceive it in his mind's eye, he would presumably fluctuate normally. In a very real sense mental output might be normal and undisturbed. But by making him *write* the alternated symbols an alien rhythm is being imposed. The normal system of switchings in meaning is being made to fit into the quicker minor cycle of pen or pencil movements, and unless that major rhythm of the mind can be brought down and interlocked with the imposed cycle, the subject will keep losing his 'meanings'. His pen will have to stop while he attempts to refocus, such refocusings quite possibly meaning a restarting of the 'set' with consequent initiating of fresh threads, and so on. (The curve may tend to take on what in a moment we shall call type B characteristics, save that it will be disturbed by random halts and losses.) The natural tendency in such cases is to weld things into the smoothest possible Gestalt. One way is to make SƧ into a THIS-that, thus welding two symbols into a single new one, the accent emphasizing the unity rather than weakening it, the giving of a neutral name still further removing the situation from that of possible conflict with the old and unidirectional name S. But that is not exactly the present point. Our concern is to note that in a test of the kind the experimenter is upsetting something not disturbed in more normal 'sets' or not disturbed as insistently at any rate, namely the natural swinging of major and minor rhythms, or (if we may call them so) major and minor meanings. So that the problem of inertia tests is not even as simple as it might seem from our discussion in terms of multidirectional facilitation. A full explanation awaits an elucidation of the mechanism of switching; of the alternation of meanings at the focus of attention.

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[1] In drawing a curve of fluctuation at the threshold we normally draw a horizontal line, recording durations during which the stimulus is in awareness as rectangles above that line, durations of unawareness being given as rectangles below. In similar fashion the record of a test in ambiguous perspective can be given by treating one of the aspects as positive and the other(s) as negative.

If we could take account of minor fluctuations, further realizing that our rectangular 'crests' and 'troughs' ought really to be made more wave-like, we get the conception of a freely moving wave form through which is driven what may be called the (horizontal?) threshold line, or line of change in meaning, namely the line mentioned at the beginning of this note. Suppose now, by decorating the drawing of the truncated pyramid, we drive that line of major meaning lower and lower until it is finally free of the curve of fluctuation. Clearly, we shall symbolize a state of affairs in which just one major meaning is present, when all fluctuations are within it, as, for example, from the corridor to its walls, to its pictures, its windows, and so on.

The problem is a complex one. We may have merely changed one line of major meaning for another. Or we may have eliminated it for all practical purposes. It is difficult to say. (Thus, for example, in doing a test in multiplication, the task is above the threshold all the time. But there may be major switches in the sense that people change from one way of 'doing' the sums to another, sometimes just 'knowing' that $9 \times 2 = 18$, sometimes arriving at the answer by addition $9 + 9 = 18$, sometimes saying the multiplication table, and so on.) However, the present point is that in an inertia test we are upsetting not merely the minor rhythm of doing a given reversed symbol, but also interfering with possible major cycles, and we ought perhaps to take both into account in any theoretical analysis of the processes involved. In other words, output curves ought to be obtained and studied for possible rhythms.

V. THE CURVE OF OUTPUT

Turning to problems of the curve of output, note must be made of a few attendant difficulties. The first is obvious, although not often explicitly mentioned, namely, that measures are of amounts of work done in successive finite time intervals. A curve so expressed may or may not approximate in outline to the underlying curve, i.e. an idealized curve representing the true ebb and flow of capacity.[1] The difficulty of assessing weight or size in fundaments, or degree of clearness, has already been mentioned. So also has that of the difference between fluctuation in rate of output, as distinct from that of rise and fall in ability. A kindred problem is that of fluctuations from one meaning to another, or from what may be called the official task or chain of fundaments to another chain. For the key fact is that of succession, and there is no way of ensuring that alien items shall be prevented from coming to the focus of attention. Any given output curve is therefore the curve of output in the official task. It is not an expression of all the mental operations occurring during the test period. By the Spearman principle of span the mind tends to keep its simultaneous output constant, quantitatively, no matter how it may vary in nature or kind. But clearly, some of that output will escape our quantitative measures. To that extent, our assessment of k will suffer, and so on. Problems of fatigue also arise. Is it that our various coefficients must be assumed constant only in the unfatigued state? Can we assume that there will be even a few seconds of unfatigued output? Shall we introduce coefficients to express the extent to which fatigue affects the results?

The curve of output (Fig. 1) is roughly hyperbolic. (It is not exactly so, but it will be convenient to use the term.) In other words there is a decrement, usually taken to be a manifestation of fatigue. Amplitudes are least on the steepest part of the curve, thereafter increasing in swing, again a manifestation said to be due to fatigue. It follows at once that measures given in our various formulae are true only as averages computed over a given length of curve. They depend upon the time T of formulae (1)–(4). The question arises whether we can express them independently of T. One way out is of course to state limits or paths. If R moves in a hyperbola, or in any other definite path, we only need say so.

It is necessary here to stress the fact that an output curve is a bundle of 'threads'. For, unfortunately, threads may start at any convenient change point, even within any one sitting at a task, a fact mentioned in a footnote to p. 198. Two extreme forms of curve may therefore be suggested. Type A is that of a single homogeneous task within which no threads are engendered save at the nominal moment of starting work. It will presumably be of marked hyperbolic shape, with geometric periods,[1] and increasing amplitudes. Apparent fatigue will be maximal. Type B is of a succession of relatively short operations, each of which calls for, or means, the setting up of new threads. The resulting curve will obviously be a succession of short hyperbolas joined end to end. To the extent that all the threads change with the commencement of each short operation, the said resulting curve will be horizontal with reference to the base-line. While being a succession of hyperbolas, it will appear to be of 'saw-toothed' outline, the said teeth having minor geometric waves on their slopes, these of course being the true attention waves, although to the eye the 'saw-teeth' may be the essential fluctuations. The said 'saw-teeth' will, however, be of constant or arithmetical (not geometric) period, and of constant (not increasing) amplitudes, save that there may be some slight wobbliness towards the end of the sitting.[2]

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[1] Thus, for example, if we are assessing by amounts per 5 sec., and if there is a wave of 5 sec. period in the underlying curve, it will not be apparent in the record. If there is a wave of 6 sec. period it will be almost completely lost, save for a vague cycle of changes that repeats itself in a period equal to the L.C.M. of the period and the assessment interval, i.e. in $5 \times 6 = 30$ sec. Only when periods are long compared with the interval will they appear apparently unchanged. Similar considerations apply in the case of amplitudes. Even if of marked extent in reality, they will be almost completely damped out if the period is approximately the same as that of the interval.

Having thus defined two extreme cases, and noting that curves obtained in practice will probably lie in intermediate positions, we may turn to a point already made in my description of a theoretical curve ((2), p. 232). In effect it was there shown that if we assume a perfect or unfatigued thread of output to be of constant or flat rate of output per *log time unit*, assuming further that each such thread carries a wave or waves of constant period and amplitude, we shall at once arrive at a hyperbolic outline, geometric periods and increasing amplitudes if we convert the said thread to a *per second* basis, the apparent change in the nature of the material being merely a matter of the method of assessment.

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[1] The conception I have elsewhere put forward (1, 2) is that waves in the output curve are geometric in period, successive crests (or troughs) appearing at times that are in geometric progression. The simplest way of dealing with such waves is to plot them against log time, their periods then seeming to be of constant length, periods for which we can give a log measure. All such log periods are integral multiples of a unit, namely, 0·0016. It should further be noted that when an empirical output curve is averaged over geometric periods, its amplitudes become constant also. It has therefore been suggested that the underlying amplitudes are probably constant, for if periods are actually constant in what may be called log time, we ought to express amounts of output per log interval, and not per second or other interval of ordinary time. By taking the empirical figures, and averaging over geometric (or log) intervals, we probably treat the end part of the curve just as the earlier portions have been treated by our method of assessment. In other words, we damp down the end of the curve to match the experimental damping occurring near the origin (see footnote, p. 214).

[2] There is here suggested a typical repetition task as so defined in industry. I have not examined a detailed curve of the kind described, i.e. one designed to show fluctuations within each cycle of change. To that extent the present description is hypothetical, although obviously logical. Research is required on the point.

For consider the interval lying between zero and 1 sec. in ordinary time. The log of zero being minus infinity, and output being (by hypothesis) proportional to the number of log time units covered, it follows that output over the first second will be of infinite amount. The intervals from 1 to 2, 2 to 3, and succeeding seconds, will all be finite, and since they cover fewer and fewer log time units, output per second will seem to fall, and rapidly. In other words the curve will take on a hyperbolic outline, despite the fact that it is flat or horizontal when plotted per log time unit. In similar fashion can be explained the fact that periods and amplitudes constant in log time will seem to become longer and longer, or more and more widely swinging as time goes on (cf. footnote 1, p. 218).

In so arriving at the three characteristics of the output curve as commonly met with, we are merely illustrating its nature as interpreted according to a logarithmic law. The analysis may or may not be sound. In its favour may be urged the fact that it brings the three characteristics under one expression, exactly covering one of them (periodicity) and at least giving approximate expression to the facts as far as decrement and increasing amplitudes are concerned. It is in these two respects that difficulties will arise, for the notion applies only to a curve of type A. With that proviso, let the analysis be assumed for a moment. We at once get a formula for the curve as given in amounts per unit of ordinary or solar time:

$$y = K . \log \frac{t+a}{t-a}, \qquad \ldots\ldots(14)$$

where y is the amount of work done in an interval of $2a$ sec., the said interval being centred at a time t sec. from the commencement of work, K being constant for the individual, and covering coefficients a, b, c and k of our earlier formulae.[1]

The formula so given is for a single thread, or a bundle of threads having a common point of origin. In practice we are hardly likely to meet with a pure example of the kind, it being so completely of type A. As soon as a curve takes on any of the characteristics of type B, elements proportional to ordinary time will enter. Whence, a more probable formula for ordinary cases is

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[1] Relationship with ordinary output curves is easily demonstrated. If a curve of formula (14) is plotted on paper graduated in logs both ways its points will fall nearly enough on an inclined straight line, save for the first point, which will be at plus infinity. In similar fashion, a work curve statistically strong enough to be steady (i.e. based on enough records) will also have its points disposed closely about a straight line, save for the first point, which although not at infinity will nevertheless be high, and quite definitely on the positive side of the line joining the other points (cf. footnote, p. 222).

$$y = K . \log \frac{t+a}{t-a} + C, \qquad \ldots\ldots(15)$$

where C is a constant amount per $2a$ sec. Moreover, the more a curve tends to be of type B, the greater C becomes, relatively speaking, until in the limiting case we get a curve purely of that type, with formula $y = C$.[1]

If this analysis holds, if output in any given thread is by a constant amount per log time unit, our unit of rate R takes on a very definite meaning. The unit of periodicity of my earlier articles (1, 2) is 0·0016. This is a value apparently true for all people. It has a very obvious connexion with the rate of switching from one mental item to another. Let therefore a unit rate in log time be one fundament brought to unit clearness per log time unit of 0·0016. Converting from this constant rate in log time to a per second basis (formula 14), let the number of fundaments (per thread) in successive seconds be $R^*_1, R^*_2, \ldots, R^*_n$. That is to say, let the number of log time units in successive seconds (from zero) be so represented. It follows that the mean rate per second over a test lasting T sec. will be the mean of the R^* values concerned. Whence, returning to earlier formulae:

$$\text{Rate}_x = c\ (abk)_x R = K_x \frac{S\,(R^*)}{T}. \qquad \ldots\ldots(16)$$

A somewhat different conception, and one bringing in fluctuations, can be obtained from the article on a theoretical curve already quoted (2). Besides assuming that any given thread is represented by a straight and horizontal line in log time, it was further defined as being periodically interrupted by troughs of unit depth and width, the periods in different threads running 2, 3, 4, 5, ..., N log units respectively, i.e. by whole number multiples of the said unit. If now all threads come to a common trough at a time

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[1] Curves from performances such as computation are dominantly geometric, with marked decrement, and increasing amplitudes, so that to the eye they are markedly of type A. But there may nevertheless be quite a large C element present, despite apparent absence of a saw-toothed outline. The curve may consist of a main bundle of threads running continuously and associated maybe with the 'set' that keeps the mind on the task in hand. If, following the usual practice, subjects make a mark or start a fresh line at given time signals, new threads may be engendered. The tiny hyperbolas so set up may be confined within the intervals concerned, thus not appearing in the curve, save as a constant amount of output per interval. Even the single items of work (single acts of computation or the like) may see the setting up of threads, still more being added to the C element.

$$T_0 = 4{\cdot}076 \times 10^{-23} \text{ sec.}$$

(the calculated common trough point for waves as met with in practice) it follows at once that we can arrive at the number of threads with troughs at any given point in the log time scale. Thus, calling T_0 zero there will be five waves with troughs at a point 12 units from zero, namely, waves of periods 2, 3, 4, 6 and 12. By definition each trough is of unit depth. So that the depth of the curve at any given point is given by merely calculating the number of factors in the (log) time value concerned. Converting to a per second basis, a curve is obtained showing the three main characteristics of a type A curve as already described (decrement, geometric periods, increasing amplitudes) with, in addition, that of constancy of phase. Redefining, the mean rate per thread becomes

$$\text{Rate}_x = K_x . \frac{S\,(R^* - F^*)}{T}, \qquad \ldots\ldots(17)$$

where R^* is as before, namely, a rate of one fundament per log unit over the interval covered, while F^* is the mean number of troughs per thread in the interval covered. (If, for example, an interval of 1 sec. covers 59 log time units, the flat rate for that interval will be 59 fundaments per second. If there are periodic interruptions of unit depth and width, that flat rate amount has obviously to be reduced by one unit for each of the troughs within that second, the said number of troughs being of course a mean value for the N threads concerned.)

As a final point: Discussion for the moment has been of single threads or bundles of threads having a common starting point, the curve thus being of type A. In practice, curves are more likely to contain a constant C (formula (15)). Without going into details, it may be said at once that this constant value would naturally be split between a, b and c coefficients, or even brought in part under k:

$$\text{Rate}_x = K_x \frac{S\,(R^* - F^*)}{T} + C = c\,(abk)_x R. \qquad \ldots\ldots(18)$$

In the extreme case, where $y = C$, the curve would have to be split into a system of successive threads or little hyperbolas, as in fact was done when it was described a moment ago as a saw-toothed complex, a logarithmic expression needing to be given for each 'tooth' separately.

VI. FATIGUE MEASURES

Correlational analysis has so far failed to uncover any factors of importance in the field of fatigue measures, although the common belief is quite definitely that some people are more easily fatigable than others, thus implying a general factor. It is clear from what has been said of the curve of output that general tendencies might be difficult to measure, while since new threads can start at any given change point, there may well be curious manifestations seeming to imply loss or transitoriness of fatigue, when what has probably happened is that fatigued mechanisms have been retired from service. This problem is especially of importance in the case of tests made by the 'interpolation' method.

If we arbitrarily define fatigue in terms of the observed decrement, assuming for the sake of argument that we have a set of pure A curves under study, there will be no fatigue at all on a log time basis, although if we convert to ordinary time a marked decrement will appear. Since people of high output will fall more (absolutely) than those of low output, it may seem that they fatigue more, unless relative measures are made, in which case individual differences will disappear. To the extent that curves take on B characteristics, individual differences may appear, although since people may conceivably vary in the type of curve produced in this task or that, specific differences may be strong, and general tendencies weak. These comments apply to the decrement. The argument is the same if increasing amplitudes are used as a measure.

The definition of fatigue as so given is in terms of the logarithmic law. It is conceivable that we ought rather to seek deviations therefrom. It will however be clear that with curves of mixed A and B characteristics, such deviations might be impossible to measure in practice.[1]

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[1] Fatigue measures in practice are usually of the observed decrement. It may therefore be noted that one useful point at least arises out of the discussion. If curves plot out more or less as straight lines on paper graduated in logs both ways, then at once we get a linear expression for the said decrement. It can be taken as the angle at which the line descends, or in some such convenient fashion. A measure so determined has obvious advantages over the more orthodox procedure of comparing output over fixed (and short) intervals at the beginning and end of the curve, for it depends on all the points (save the first one) and not on a few alone. Since the above was written, measures of the kind have been made by Birtwistle (12).

VII. SUMMARY

An attempt has been made to harmonize the two-factor theory of ability with that of performances as determined by threads of output occurring at random. Formulae have been put forward expressing output in terms of rate coefficients a, b, c and k, where k measures a general tendency and is constant for the individual, a being specific, b and c representing group elements subject and not subject to individual differences respectively. Following Spearman's use of the concept of energy, the meaning attached to kR is that of 'mental horse-power'. The interpretation of g as identified with eduction is not, however, adopted, the nearest justifiable approximation seeming to be a definition of such 'horse-power' in terms of unitary fundaments moved through unit clearness in unit time.

Referring to the conception of g as identified with eduction (as distinct from reproduction) it is suggested that if g measures or represents horse-power or energy, and if mental objects can come to clearness by two distinct processes (eductive and reproductive) then measures of those processes must be given in group and/or common factor form. If it is not desired to systematize in such fashion that eduction becomes a group or common factor, it would seem that we must make it cover all coming to clearness whatsoever, thus (in effect) dropping the eduction-reproduction antithesis.

The problem of expressing variations in g saturation in terms of an invariable energy or horse-power is discussed, it being suggested that variations are on the specific side, needing to be expressed by diminishing a and b coefficients to form a c coefficient constant in size for the group of persons under test. Group factors in the correlational sense are traced to b coefficients. Formulae are given expressing measures of o and p (oscillation and inertia). In discussing p as measured by reversing performances of marked forward or unidirectional facilitation, it is noted that performances of high g saturation are of multidirectional facilitation, it being difficult to measure p with them. The curve of output is discussed in terms of two extreme types, the first being fundamental. Taking its (roughly) hyperbolic outline and increasing amplitudes as manifestations deriving from a logarithmic law, formulae are given to convert to amounts per second. Finally, a note is made on the matter of fatigue measures in the light of these curve types.

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PUBLICATIONS RECENTLY RECEIVED

A Guide to Mental Testing. By Raymond B. Cattell. London: University of London Press. 1936. Pp. xvi + 312.

When a person enters the world of practical mental testing he is apt to feel appalled by the apparent impossibility of choosing out of all the available tests those which will be most satisfactory in giving him a rapid and reliable measure of mental characteristics. Is he to plough through the four thousand tests listed in Hildreth, becoming familiar with each one and trying it for a time before deciding whether or not he thinks it is a good test? To do so would be obviously fantastic, but if he does not do so how can he decide which tests are the best to use? In practice what he usually does is to take those tests which his predecessors have used and which are available at the place where he does his testing, become familiar with them, and gradually extend his experience by experimenting with other tests. There has, however, always been a need for a book that will introduce him fairly soon in his career to some of the tests that an experienced tester has found to be most valuable in studying the varied aspects of personality, and Dr Cattell's book attempts to fill this gap. From this point of view it is a very useful contribution. Here the tester will find gathered together in a convenient form, together with up-to-date norms, a selection of verbal and non-verbal intelligence tests, performance tests, tests of special aptitudes, attainment tests, interest, attitude, temperament and adjustment tests, rating scales, inventories, and questionnaires. And most of the tests described in greatest detail have been devised by Dr Cattell himself.

The main omission in the book, as Dr Cattell points out, is the lack of any detailed reference to the Binet scale, which is probably used more frequently in clinics than any other test. One reason for this is that Dr Cattell can find "no scientific evidence for its being as valid as most of the tests devised since Spearman's principles were discovered" (p. xv). Another reason is that although "it is certainly true that the examiner gains evidence of temperament and character traits in administering the Binet intelligence scale which he would not gain with paper tests, (yet) when definite temperament tests are available it is a mistake to vitiate the intelligence test by roughly blending the two" (p. 7). Again, "devotees of the Binet test whilst admitting its deficiency in '*g*' will sometimes argue that it is yet superior to the tests here advocated, in which the subject works for several minutes at a stretch on his own, because of the close 'rapport' between tester and tested. But this 'rapport' is in many cases nothing more than the rather tense atmosphere in which the experimenter is insistently firing questions at a fundamentally unwilling subject" (p. 268). This makes one wonder if the subject is any more 'willing' when he is confronted by a booklet containing a couple of hundred questions which are fired off at him simultaneously. It makes one wonder if Dr Cattell believes in a new faculty psychology with temperament on tap to be turned on for the temperament and off for the '*g*' tests. If he does not, what objective and statistical method does he use in estimating the effect of the subject's temperament on his performance in the '*g*' test? If there is none available and he is obliged to guess at this when writing up the case study, where is the basis for his criticism of Binet? The latter in fact appears to win hands down, for the effect of temperament on intelligence (assuming it is possible to separate these two aspects of personality) is there studied in an integrated instead of in a dissociated way.

Two other points deserve mention in this connection. The first is the question of time. Usually in a clinic—deplorable as it may be—the chief trouble from the point of view of the tester is lack of sufficient time to give all the tests he wants to give.

By giving the Binet scale he can get a more rapid outline of the subject's personality than by giving '*g*' tests followed by temperament tests followed by probes of character. "Possibly," Dr Cattell might reply, "but not nearly such an accurate one." This leads us to the second point. In spite of what might be expected on theoretical grounds the astonishing thing about the Binet scale is, in my experience, its remarkable consistency, both when the same tester does a retest on the same subject at a later date and also when a different experimenter does one.

These points have been elaborated at some length, for they are fundamental one is to understand Dr Cattell's approach. On the one hand, it might be argued that the most satisfactory and practical method for clinical purposes (and mental testing is primarily a clinical device) is first to provide a series of standardized situations which will give some indication of the personality as a whole, and then to follow this up with further investigations of more specific aspects that appear to need fuller investigation. On the other hand, there is Dr Cattell's approach which is to provide a series of tests for certain statistical quantities, and then to infer the personality from a combination of these test results. But whichever method of approach is the more sound, Dr Cattell's book provides a very handy work of reference for those who are engaged in practical testing.
J. M. B.

Colour Science. By WILHELM OSTWALD. Translated by J. SCOTT TAYLOR. London: Winsor and Newton, Ltd. Part I: pp. xviii + 141. 15*s*. Part II: pp. xii + 173. 10*s*. 6*d*.

It is to be expected that English psychologists will welcome the appearance of these volumes (the date of publication of which is not given), the first translation of the late Dr Wilhelm Ostwald's *Farbkunde*. When so famous a chemist and physicist as Dr Ostwald enters a domain properly that of psychology, as he himself points out in the introduction to Part I, one is justified in hoping at least for an unusual approach to this difficult subject. In this one is not disappointed, for, after an adequate introduction to the history of colour theory, a short and concise statement of the nature of light, and a brief outline of the processes of vision, Dr Ostwald's first task is to urge us to adopt, without quibble, the Weber-Fechner Law. This he does in order to permit the establishment of a fixed 'Grey Scale' or series of achromatic standards. This scale, it is contended, should be so arranged that each individual grey is psychologically equidistant from its immediate neighbours; in short, each series of three adjacent patches should represent an example of two 'equal-appearing intervals' of brightness. Unfortunately, however, the series of 24 greys finally selected, designated from the whitest to the blackest by letters of the alphabet, is chosen from the *theoretical* Fechner, or logarithmic, scale. The obvious fact that Weber's Law is disobeyed both for very high and for very low light intensities leads to the clearly apparent result that, even under normal conditions of illumination, the various greys at either end of Dr Ostwald's scale are never even approximately equidistant.

This example of Dr Ostwald's treatment of the achromatic colours is sufficient to indicate his mode of procedure when dealing with the phenomena of hue. He first points out that the "Subtractive Primaries" are a "delusion and a snare", and indeed he rejects the possibility that any adequate colour teaching can be based upon consideration of the colours of the spectrum. Dr Ostwald, therefore, is wholly concerned, except for purposes of standardization, with the hues remitted by surfaces of various kinds. He proceeds to arrange the so-called chromatic circle in just such a manner as was adopted for the achromatic scale. Twenty-four standards of hue are selected, and all but numbers 7–11, the extra-spectral reds and purples, are defined in terms of the wave-lengths of their spectral counterparts. As such, therefore, this scale represents a useful, and almost wholly reproducible, standard scale. But, at least to one observer, it could scarcely be argued that the hues are psychologically equidistant.

In Part II of this work, Dr Ostwald describes various pieces of apparatus employed for the verification of his suppositions. It would have been of great advantage if somewhat more adequate drawings could have been provided in this section of the work, for the instructions given are hardly sufficient to permit the reproduction of the original apparatus in other laboratories. In the next section, the effects of mixing colours, and the nature of various pigments and binding media are described. (In this connexion, surely white of egg and not yolk is intended on p. 108.) Lastly, after a brief chapter on the Physiology of the Eye, which, as it is only ten pages in length and is not in any way illustrated, seems scarcely worthy of inclusion, Dr Ostwald embarks upon a discussion of various aesthetic laws which arise from consideration of his scales. Representative of these, discussed also in Part I, is that concerning grey harmonies. On p. 135 of Part II we see that "Single, Double and Triple-stepped Harmonies have been established according to the intervals between the members of the Grey Series. The single-stepped harmonies are *ace*, *ceg*, *egi*...", in short, every other grey on the scale. The double-stepped harmonies, as is indicated, are *aei*, *cgl*, etc., the triple-stepped *agn*, *cip*. "I have before me", says Dr Ostwald on the following page, "six large frames in which twelve such specimens are arranged. I have looked at them hundreds of times, and the delight they give me is not yet exhausted." A similarly gratifying result may, it is claimed, be secured by appropriate selection from the Ostwald Colour Solid.

Finally, on p. 166 and onwards, we are encouraged to replace our paint boxes by a "Colour Organ". The so-called "Powder Organ" which Dr Ostwald himself favours, consists of 680 standardized colours, in powder form, each in a separate China-ware pan, together with a suitable binding medium. At its smallest, assuming that each pan and its contents weighs ½ oz., such an organ, without container, would weigh about 21 lb., and seems a clumsy alternative to the normal paint-box.

To what extent Dr Ostwald's achromatic and chromatic scales are worthy of general adoption as standards is a problem which can only be indicated here. In particular, however, there should be no difficulty in rearranging the colour circle in such a way that the hues were in fact psychologically equidistant. A. F. R.-S.

Studies in Psychology from Smith College. Ed. by J. J. Gibson. Psychological Monographs, No. 210. Princeton, N.J.: Psychological Review Co. 1935. Pp. 98.

Dr Gibson's earlier work on perceiving and remembering was notable for the caution and impartiality manifested in all inference from experimental results. These admirable qualities are likewise represented in two papers on the orientation factor in visual perception, the first in collaboration with D. Robinson and the second with M. Radner. Gibson and Robinson investigated the influence of the spatial orientation factor upon the recognition of familiar forms. Recognition of such forms is shown to depend in large degree upon their habitual spatial orientation, deviations from the latter tending in varying degree to prevent recognizing. In interpreting their results, the authors accept Koffka's conception of a spatial framework, but believe learning to constitute the major factor in determining that particular orientation of a form experienced as normal. Radner and Gibson studied the reproduction of forms exposed at an angle. Their results are very clear-cut: forms objectively at an angle tend to be reproduced as upright, but if this discrepancy, or 'tip-character', be noticed, then the latter will usually be exaggerated in the reproductions. In order to explain these results, the authors postulate a 'perceptual centre' of normal orientation. This they regard as a product both of past experience and of dynamic factors tending to stability. The whole hypothesis, though a happy compromise between associationism and Gestalt theory, must inevitably suffer from the shortcomings of both doctrines.

One of the most heartening signs in contemporary experimental psychology is the shift of emphasis from the more static to the more dynamic aspects of mind, and such a trend is well represented in this volume of studies. E. Siipola's first paper records some investigations of the operation of attitudes or 'preparatory sets' in perceiving correct, misspelt and meaningless words. Though the effects of such attitudes in determining perception have been long known and variously interpreted, her experiments do indeed demonstrate such effects in a novel and convincing manner. Moreover, they bring to light the interesting fact that under certain conditions preparatory sets which had been established in one task would 'carry over' to affect the performance of a subsequent task for which no set was prescribed.

Rees and Israel report a closely similar study of the influence of such attitudes upon higher types of mental process, notably the solution of anagrams. They also studied the genesis of such 'mental sets' and attribute to experience or training as potent a rôle in this respect as is played by the more usual method of specific experimental instructing.

The volume includes also a study in the psychology of mirror-drawing by E. Siipola and an account by M. W. Curti of the responses of white rats in the presence of cats. In the case of the latter investigation, it is indeed gratifying to observe that for once scientifically controlled observations tally remarkably closely with the everyday expectations of the layman.

Experimentelle Psychologie in Demonstrationen. Von E. BRUNSWIK. Wien: Julius Springer. 1935. Pp. x+166. RM. 10.60.

Here is a very excellent informative survey of experimental psychology. The topics are discussed and illustrated in the following order: the Special Senses (far and away the greater part is devoted to a consideration of the phenomena of vision); Form Perception; Spatial Perception and Objectivity; Memory and Thinking; Feeling; Activity; Personality Types. The exposition is clear and comprehensive, a good choice of experiments is given, and there are adequate references to other German authors. So far as the range of topics goes the book may be safely recommended to any student who wishes to have a reasonable introduction to contemporary German experimental psychology.

Wahrnehmung und Gegenstandswelt (*Grundlegung einer Psychologie vom Gegenstand her*). Von EGON BRUNSWIK. Leipzig und Wien: Franz Deuticke. 1934. Pp. xi+244.

"Psychology with reference to the object" is meant to be the inquiry into the 'objective achievements' of psychological processes. In the present book Brunswik formulates the principles and results of a battery of experimental investigations inspired and guided by the author and either published or to be published elsewhere. He is particularly interested in the question as to how far perception is capable of "attaining aimed-at objective properties" (*intendierte Gegenstände zu erreichen*). For instance, in the case of constancy of phenomenal size this would mean to investigate the extent to which the real size or the projected size is perceived, if the one or the other is aimed at. Since, as a rule, the object selected is found to lie between the two "poles of intention", he calls it the "mean-object actually attained". In the ordinary illusions also, mean-objects only are perceived; for instance, if one tries to match the number in a given pattern of small coins with a set of bigger ones, the result is an underestimation of number, as if the subject were involuntarily trying to match the area (or the monetary value) as well. Brunswik seems to imply that all illusions arise from such an involuntary deflexion towards "foreign poles of comparison". This amounts to a claim that, if several objective properties are working together, the phenomenal result must needs be a 'compromise' of the single effects (an attempt as ingenious as unwarranted to rescue the old hypothesis of constant connexions between stimulus and response). Later on in the book (Abschnitt 6) all the factors found—by co-variance—to influence a given phenomenon are called "coherent poles of intention", implying that a cue-like retinal disparity in binocular vision is also something aimed at, but not attained (cf. p. 190). One wonders what is supposed to be the *psychological* significance of the concept "pole of intention", if such items as the following are subsumed under constancy of size: projective size, real size, disparity and distance. (On p. 161 overcompensation, which sometimes occurs when the more distant one of two equal objects appears bigger, is interpreted as a-mean-object between projected size and distance! (Cf. also p. 170.)) It is difficult to see the connexion between this sort of "psychology with reference to the object" and psychology.

La Mémoire est Perfectible. Par GEORGES LÉOTARD. Bruxelles: Les Éditions Marguerite. 1935. Pp. 84. 7.50 frs.

This is an experimental study carried out in schools in Brussels. It is maintained that the development of vision has played the most important part in the growth of the great brain. Since memory is one of the most important of the cerebral functions, it should follow that keen and accurate vision is positively correlated with a good memory. The author claims that his experiments demonstrate this to be the case, and show that whatever means can be employed to extend the field of vision and to improve visual discrimination directly facilitate correct recall.

Clear Thinking: An Elementary Course of Preparation for Citizenship. By R. W. JEPSON. London: Longmans. 1936. Pp. x+179. 3*s.* 6*d.*

Though this book is not written in the name of psychology it contains some very sensible psychological observations, and a mass of excellent and original illustrations from which any psychologist will be able to extract material to aid him in his hunt for the principles that are present in thinking processes. From a general point of view also the book has a lot to be said in its favour. Nobody can learn to think clearly out of a book, but anybody can be helped to do so, and this book ought to be of assistance. Rightly the main aim has been to exhibit wrong ways at work, so that a reader may be prepared, at least, to know the common fallacies when he meets them.

Adult Intelligence. By THEODORE WEISENBURG, ANNE ROE and KATHARINE E. MCBRIDE. London: Humphrey Milford, Oxford University Press. Pp. xii+155. 6*s.* net.

This is a report of an extensive study of adult intelligence carried out on patients in three Philadelphia hospitals. The tests used included language intelligence tests, educational achievement tests, and non-language tests. The patients, seventy in number, were carefully selected so that only those who were mentally normal were accepted.

The main conclusions are that whereas some of the language tests, particularly the sentence completion and the vocabulary tests, are suitable for discriminating between normal adults, many of the performance tests fail to discriminate between them. Mental development, tested by the former, is shown to continue through the twenties and the thirties and to show only a small decline thereafter; that tested by the latter shows an early peak, probably falling either in the twenties or before, with successive declines in the thirties, forties and fifties. It is suggested that the effect of education on test performance is not as important as it has often been thought to be, and that intelligent individuals who have had only a poor school training often compensate for this latter in adult life, whilst the dull individuals who have had more school training than their intelligence warranted either do not profit by it or else tend to forget what they once knew.

Wirkungsweisen von Farbeindrücken auf schwachsinnig Kinder. Von MAX WITTMAN. *Forschungen über Farbspielkunst.* Bd. I. Heft 2. Herausgeben von R. H. GOLDSCHMIDT. Leipzig: S. Hirzel. 1933. Pp. 121.

Feeble-minded children were made to judge the pleasantness of dual colour combinations under varying conditions, e.g. with resting and shifting colours ('Farbwandel'). Comparison with normal children was scarcely attempted!

The Psychological Aspects of Child Development. By SUSAN ISAACS. London: Evans Brothers, Ltd. 1935. Pp. 45. 1*s.* 6*d*. net.

This pamphlet offers "a bird's eye view of the most significant recent advances in the psychology of infancy and early childhood". It should form a good introduction to further studies. As all psychologists now know, Dr Isaacs has a mass of original observations on which she can base her studies, and this, coupled with a wide knowledge of the literature, and a sympathetic and sane outlook, make her unusually well qualified to lead any student into this field of research.

Conversations with Children. By DAVID and ROSA KATZ. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. xii+318. 10*s*. 6*d*. net.

This book is based upon the verbatim reports of 141 conversations selected over a period of a year; they took place between two boys, aged 3–4 and 5–6 years respectively, their parents, Prof. and Dr Katz, and occasionally certain other adults and children. The conversations are sometimes initiated and frequently guided by the adults, but there is also much spontaneous talk and questioning by the children. Taken as they stand the conversations provide much interesting psychological material. But the tendentiousness of some of the detailed comment upon them compels one to doubt the scientific impartiality of observation and experimentation of the authors, and to fear that they have been too much inclined to "point the moral and adorn the tale". The theoretical discussion given in the Introduction and in Part II, General Results, deals with the light thrown by these conversations upon the child's relation to his external environment, his method of speaking and thinking, and his ideas of reality; this discussion is interesting without, at this date, being very original (the date of the original German publication is not given, and may have been considerably earlier).

The Development of Emotion in the Infant. By WILLIAM E. BLATZ and DOROTHY A. MILLICHAMP. University of Toronto Press; London: Humphrey Milford. 1935. Pp. 44. 3*s*. net.

A Study of Tics in Pre-School Children. By WILLIAM E. BLATZ and MABEL C. RINGLAND. University of Toronto Press; London: Humphrey Milford. 1935. Pp. 58. 3*s*. net.

These two monographs are Nos. 3 and 4 of the University of Toronto Child Development Studies. The first reports the results of a systematic observation of five children for three consecutive days of each month from the first month to two years of age. The records were kept by trained parents. It is interesting that the emotional situations recorded are practically all 'thwarting' situations, and it is maintained that "the emotional life of an organism starts when the experienced situation cannot be adequately dealt with by means of the response repertoire available at the time". The earliest kinds of emotional behaviour appear to be of a sort termed 'anger' and 'fear'; more complicated behaviour is developed on a basis of these according to the kind of conflict aroused.

The second report rests on the observation of twenty-five nursery school children on two occasions each. Tics of one kind and another were exceedingly frequent. They were most commonly connected with the mouth, and most apt to appear when gross bodily movements were prevented. Further research is indicated.

Ruth. Tagebuch über die Entwicklung eines Mädchens. Von VILHELM RASMUSSEN. München und Berlin: R. Oldenburg. 1934. Pp. 300. RM. 7.50.

This book consists of an account, kept by her father in the form of a diary, of a girl's life from birth to 17 years of age. The entries were made sometimes daily, sometimes every few days. They describe during the early years physical traits and behaviour, perception, memory, intelligence, activity, etc. Later, the majority of items consist of remarks and questions made by the girl, but drawings, written compositions and results of intelligence tests are also included. The book is conveniently annotated by marginal titles referring to the behaviour traits indicated. No analysis of the material is attempted.

Die Dankbarkeit bei Kindern und Jugendlichen. Von FRANZISKA BAUMGARTEN. Bern: A. Francke AG. 1936. Pp. 106. Fr. 4.80.

This is a study of gratitude in children, a subject of obvious importance in social psychology, which so far has received little attention. Dr Baumgarten started her investigation by developing an experimental technique for studying the matter and from this proceeded to classify the different modes in which it may be expressed. This is followed by a study of individual differences at each age level, and of age differences.

Jugend als Konfliktsituation (Jugendpsychologie mit Berücksichtigung des jüdischen Kindes). Von WALTER BLUMENFELD. Berlin: Philo. 1936. Pp. 122.

In depicting 'youth' as a conflict situation Blumenfeld is conscious of dealing with youth in modern Western societies. Here puberty is bound to cope with two main conflicts: the sexual erotic one and the generation conflict. The various trends and attitudes of puberty are interpreted as reactions to this typical situation. The book, being a summary rather than an original study, contains ample material: excerpts from diaries, curves of development as to achievement and attitude, ethnological parallels, etc. To compare it with analogous studies on English youth would be interesting.

A Handbook of Social Psychology. Edited by CARL MURCHISON. The International University Series. Clark University Press; London: Humphrey Milford. 1935. Pp. xii+1193. 27*s*. net.

What can be said about this vast book? In so large a volume it would be a marvel indeed if there were not to be found contributions of value and interest. And as a matter of fact the whole work proceeds on a high level of concentrated learning and research. Yet any reader who turns to this book in the hope of finding what psychology can say to help his understanding of the life of man in society and of current social problems will probably be very disappointed. There are six parts to the book, and they deal with the following topics: Social Phenomena in Selected Populations (two of them deal with bacteria and plants); Social Phenomena in Infra-Human Societies; Historical Sequences of Human Social Phenomena; Analysis of Recurring Patterns in Social Phenomena; Analyses of some Correlates of Social Phenomena; Experimental Constructions of Social Phenomena. One imagines the Editor bothered to death by the people who constantly cry that social psychology is all theory and no fact, and determined at all costs to dish these critics. So he casts an enormously wide net and in the result has caught an incalculable quantity of small fish. That the book will for a long time to come be of very great use to the specialized student who will read parts but not all, is certain. That it must fail to give any coherent picture of the problems, technique and promise of social psychology as a science upon which any hope for the future can be built seems also certain. This criticism may be ungrateful. It may be impossible to have both. But a good many people think that what is needed in this field is not anything like a definitive handbook, but something which will stimulate and direct research in fruitful fields. There is little of that here, though practically everybody who writes has written well about the topic which he was asked to discuss.

Trait-Names: A Psycho-Lexical Study. By GORDON W. ALLPORT and HENRY ODBERT. Psychological Monographs, No. 211. Princeton, N.J.: Psychological Review Co. 1936. Pp. viii + 171.

17,953 'trait-names', collected from Webster's unabridged *New International Dictionary* are presented and classified in this monograph. The authors are not altogether sure what the psychologist will do with them, but they suggest various things that he might do and say that "if from them he can find support for newer and better theories of personality he is welcome to do so". Even if it did nothing else the list would provide the toiling psychological writer with a magnificent lot of synonyms. But in fact the list is prefaced by a most interesting and stimulating short discussion of current theories about personality traits and attitudes. The field is at the moment a highly confused one and the authors' attempt to introduce into it some sort of order is an excellent one.

Psychodynamik des primitiven Denkens. Von F. M. FELLER. Leipzig und Wien: Franz Deuticke. 1933. Pp. 58. R.M. 2.40.

The first section contrasts a kind of static association theory of thinking such as Wundt's with a dynamic association theory, according to which primitive thinking rests largely on the utilization of a sort of magical set of words and formulae which do not mirror external fact merely but are in living touch with a psychical reality. The rest of the monograph illustrates this, very largely by a study of cave drawings and the sexual symbols in which they are said to abound. The author is greatly influenced by psychoanalytic interpretations. There is no pretence at first-hand study, and most readers may find the monograph disappointingly one-sided.

Elementares Lehrbuch der Sozialpsychologie. Von W. HELLPACH. Berlin: Julius Springer. 1933. Pp. 165.

So long as direct and controlled social observation, especially of the contemporary civilized social group, remains in its present undeveloped state, it is doubtful whether anybody could write a satisfactory text-book of social psychology. This attempt is interesting, and may be of use to students in directing their attention to numerous problems which require further elucidation. The first and greater part is concerned with socially determined reactions of the individual, and with characteristic means of communication between individuals in the group. Following are discussions of group or collective psychology. The English reader will be interested in the suggestions for further study. The whole treatise moves on a rather general and theoretical plane.

Sex and Temperament. By MARGARET MEAD. London: Routledge. 1935. Pp. xxii + 335.

The ideas put forward by Ruth Benedict in *Patterns of Culture* are carried still further by Margaret Mead in this book. Dr Mead stresses the importance of the cultural pattern in determining questions of sex-temperament. She instances that in those societies in which the behavioural norms of men and women are sharply differentiated, with either men or women in the dominating position, there is to be found the prevailing view that the temperaments of the two sexes are quite different; whereas in those societies in which the behavioural norms for men and women are similar the view prevails that their temperaments are similar also. In order to illustrate this point of view Dr Mead gives a description of the respective rôles of men and women in three primitive communities of New Guinea, the Arapesh, the Mundugumor, and the Tchambuli; and she traces the developmental aspects of these patterns of behaviour by describing the cultural settings in which the growth of the children, marriage, the relationship between the adults in the communities, etc., take place. Yet though the behaviour of the majority can be interpreted in terms of the prevailing pattern of culture, there will still be some individuals who are innately unfitted to adjust to this pattern. This Dr Mead is fully prepared to admit, as Dr Benedict was before her, and the conclusion to which they have both been drawn is that the deviant in one society may be the perfectly adjusted person, or even the leader, in another pattern of culture. All her conclusions are of considerable psychological significance.

It is only with hesitation that a criticism is made of this new and hopeful development in anthropology which opens the door to the problems in the common ground between anthropology and psychology. But why has the material not been presented in a scientific manner so that the serious reader may be able to distinguish between the *facts* and the *interpretation* that Dr Mead has put upon them? Dr Benedict did at least mention the sources of her information, and the sceptic is able to look up the facts for himself if he will. But there is nothing comparable to this in Dr Mead's book. Is it too much to ask her for a sequel in which are published her field note books, the sources of her information, the day on which she wrote down her observations together with the day on which she observed them, how far new material was added when she wrote up her results, and other matters of equal importance? It has not, to be sure, been the custom of anthropologists in the past to publish their field note books, or even extracts from them, but what an opportunity has here been missed of not only developing a new orientation to the subject but also of establishing it as a *science*!

Race, Sex and Environment. By J. R. DE LA H. MARETT. London: Hutchinson's Scientific and Technical Publications. 1936. Pp. 342. 21*s.* net.

This is a highly speculative work which aims at showing how mineral deficiencies, brought about by climatic effects, may have determined the course of evolution from the lowest forms to man. For example, the evolution of the erect attitude, the loss of body hair, the expansion of the brain and skull and the increase of intelligence are all viewed as the results of atavistic reaction to mineral, chiefly iodine, deficiency. Mineral deficiencies are shown to be possible causes of racial differences and thus to affect marriage systems and to offer a chance of understanding the cause of Kretschmer's correlation between physique and character. An offshoot of the mineral hypothesis is the substantiation of the Oedipus complex. The id, it may be remarked, is conceived "as consisting mainly of inhibited manipulative behaviour appropriate to a small social tree-dweller;...The super-ego might be equated with the unrestrained ferocity of a giant-anthropoid, [which]...has come to be inhibited by the last evolving, or so-called Oedipus, stratum" (p. 299). Included in the all-embracing net are such matters as Coyness and Taboo; Polyandry and Incest; Cannibalism and Sexual Selection; Religion and Environment; Strong Minds and Weak; Race and Education.

Both Sides of the Buka Passage: An Ethnographic Study of Social, Sexual, and Economic Questions in the North-Western Solomon Islands. By BEATRICE BLACKWOOD. Oxford: at the Clarendon Press; London: Humphrey Milford. 1935. Pp. xxiii + 624. 35*s.* net.

Miss Blackwood was given extremely wide terms of reference. Her work was undertaken for the Committee for Research on Problems of Sex, National Research Council, Washington, and it was indicated that she should cover "the whole ethnic setting, as well as biological, psychological and social implications". Undeterred, indeed encouraged, by her vast task she set to work. She spent a year in the field and this large and intensely interesting volume is a part of the outcome. Nobody could hope to do better than this what she was asked to do, and the result is of great interest to the psychologist. The daily life of the people with whom Miss Blackwood lived is described with great detail and vividness, and many scenes are reproduced in the series of excellent photographs which the book contains. As well as a great amount of detail about the social background, marriage, childhood, adolescence, food, useful and ornamental arts, economics and belief, there is some attempt—naturally not very technical—to collect some data about psychopathology in the region studied, and there is a most useful selection of dreams. As a mine of information, from which all kinds of significant psychological data can be obtained, the book is first-rate. It is less easy, amid all the detail, to get a clear picture of the life and character of the people themselves. It is qualitative, descriptive, observational work of a high order. Yet, breaking out of it here and there, it follows, on the whole, a rather conventional anthropological pattern. The social psychologist should read it. From it he will learn much about the sorts of things that can be observed and how to set to work. At the same time he will be bound to wonder whether perhaps some restriction of the field, and some attempt definitely to introduce and use controlled methods and experiment might not be in some respects a gain in the way of promoting a genuine understanding of social determinants.

Individuality in a Collective World. By BARBARA SPOFFORD MORGAN. London: George Allen and Unwin. 1936. Pp. lx+260. 10*s.* 6*d*. net.

Dr Morgan presents a very thought evoking essay on the extremely complex problem of the place of the individual in the social, collective world. She shows that individuality is not the unit from which society develops, but rather it is an acquisition that the individual achieves as he is able to integrate his experience and develop it above the restrictions of the collective world.

The book first considers the different ideas held in the East and West regarding the position and meaning of the individual in his society. Secondly, the psychological agents by which the biological organism is transformed into an individual are discussed. Here the author, using a Gestalt viewpoint, describes individuality as resulting from the resolution of basic conflicts and the re-integration of experience into new wholes at higher levels. In the third part the training by which individuality is formed, and the situations in which, once formed, it may break down are described. The further the child is trained in the routines of society the more automatic they become and therefore the freer he is of them. The more he can manage the routines, the more he is master of his society, and can integrate, rather than merely acquire, material from it. When, however, he becomes uncertain or afraid, the individual falls back to the state of the Collective Mind, which is the lowest common denominator of the ideas, attitudes and mores of the individuals who possess it, and in it he finds support in his crises.

The author's belief that individuality is irrational and qualitative, and that science, which is rational and quantitative, cannot approach it, does not make psychology useless, but merely reminds it that its investigations can deal only with the rational quantitative processes in which individuality manifests itself, and not with its essence which belongs rather to a Hegelian world of reality, which presumably can be reached, if at all, only by philosophy.

Psychologists who may be too readily persuaded to talk easily of 'the Individual' and 'Society' as if they were two things, will find the paradoxes and difficulties resulting from the acceptance of this division revealed and considered in a remarkably lucid discussion embodied in a simple and delightfully vivid style.

Outlook upon the Future of British Unemployed, Mental Patients and Others. By NATHAN ISRAELI. New York: Science Press Printing Co. 1935. Pp. 30. 50 cents.

This tells the story of experiments conducted in England and Scotland in which the subjects were asked to fill up rating-scale forms relating to what they considered to be their probable attitudes towards various future possibilities. They were, for example, asked to estimate upon a five-point scale what they anticipated about such things as "In the future do you expect to do work you like?" "Do you expect to try to help others?" "Do you expect to win fame?" "Does your future seem bright to you?" "Does your future seem dark to you?" and so on. Some like questions were put with regard to the past. The results, as they concern unemployed groups, patients in a mental hospital and high I.Q. groups, are summarized, and are held to throw much light both upon the social reactions of these groups and upon individual personality.

Human Values in Psychological Medicine. By C. P. BLACKER. Oxford Medical Publications. London: Humphrey Milford. 1933. Pp. x+179. 8*s.* 6*d.* net.

The author is convinced that the main immediate need in medicine of a practical kind is a strenuous effort to understand the personal characteristics and idiosyncrasies of individual sufferers. These, particularly in so far as affective reactions are concerned, are to be found chiefly in the person's equipment of values. Hence the physician should seriously try to understand both the nature and the determination of values.

The book is by no means easy to read, for the argument often goes far afield and roams over wide realms of philosophy, though the main shaping influence comes from the psychology of Freud. But it is worth reading. Values are classified as commercial, biological and sentimental. Difficulties in their study due to the plasticity of mental process and the corresponding shifting of meaning of terms are discussed. The social factors which shape individual values are set forth, and an attempt is made to consider the main values about which the most important individual activities cluster. A final paper deals critically with Freud's notion of Life and Death Instincts. Few readers, probably, will be carried all the way by the author, but none will fail to find his discussion stimulating.

Personality Maladjustments and Mental Hygiene. By J. E. WALLACE WALLIN. London: McGraw-Hill Publishing Co. Ltd. 1935. Pp. xii+511. 18*s.* net.

It is clear that the author of this book has expended immense labour in collecting, analysing and systematizing the material upon which the book is based. This material takes in the main the form of brief case histories and autobiographical sketches of maladjusted individuals. In Part I it is arranged to demonstrate the effects of applying a programme of mental hygiene to maladjusted persons, and numerous useful suggestions for preventive measures are given. In Part II are given detailed illustrations of the types of symptom encountered in these persons. This plan of arrangement has the unfortunate effect of leading the student to suppose that such cases can be usefully grouped and considered from the point of view of isolated symptoms. Moreover, the author's diagnoses seem to be altogether too superficial to be reliable except in the simplest cases; nor does his theoretical discussion throw much light on the underlying nature of these maladies. A less comprehensive and more selective survey of the field might have given a clearer picture of the subject.

The Achievement of Happiness. By BORIS SOLOKOFF. London: Denis Archer. 1936. Pp. 253. 8*s.* 6*d.* net.

Everybody can be happy, or at any rate everybody ought to be; but few people are. Why is this? Dr Solokoff has written a number of essays around this topic; the first five deal with Love and Happiness and the remainder merely with Happiness. There is much personal reminiscence, much about glands, and not a little advice either implied or direct. The book is said to be written "from the standpoint of an authoritative physician", and certainly it does not lack drastic and decisive statement.

The Glorious Bondage of Illness. By FRANCE PASTORELLI. Translated from *Servitude et grandeur de la maladie*, by A. D. London: Allen and Unwin. 1936. Pp. 224. 6*s.* net.

Mme Pastorelli gives an introspective account of the psychological life of a talented woman, who has been condemned to bed and chronic suffering by severe heart trouble. She shows how illness isolates the sufferer and therefore gives him an 'ego-centric' outlook. The conflicts which result she describes, and in her own case describes how a religious philosophy is capable of resolving them. She points out the unnecessary irritations and pain that unseeing healthy people cause the invalid and suggests that by a better understanding of the psychological world of sickness these could be avoided.

The book contributes nothing to scientific psychology, but as an account of an individual's development during a long period of suffering may give insight into the private worlds of those isolated from general life by illness.

Die Irrtümer der Psychoanalyse: Eine Irrlehre mit einem genialen Kern. Von HENRIK EGYEDI. Wien, Leipzig: Wilhelm Blaumüller. 1933. Pp. vii + 86.

This critique, fair but somewhat scanty, centres on 'free association' which is held to play a double rôle in psychoanalysis, being a method of setting up a quasi-hypnotical (suggestive) relation between psychotherapist and patient, and also the very medium of reasoning through which the psychoanalyst arrives at and propagates his views.

Über den Menschenhass: Eine pathographische Untersuchung über Jonathan Swift. Von ADOLF HEIDENHAIN. Stuttgart: Ferdinand Enke. 1934. Pp. 118.

This study arranges biographical facts under characteriological and psychiatrical headings, also taking into account the bodily constitution and disorders. An explanation of Swift's misanthropy is attempted, but cannot be said to link up very closely with the preceding classificatory work. Psychoanalytical ideas are made use of without being pushed to extremes of universal explanation.

Psychische Automatismen: Zur Experimentalpsychologie des Unterbewussten und der aussersinnlichen Wahrnehmung. Von HANS BENDER. Leipzig: J. A. Barth. 1936. Pp. 135.

This investigation was inspired by the use of the 'planchette.' The two elements apparently involved in such a performance, *automatic writing and extra-sensory perception,* are subjected to separate investigations. The latter is tested by covering up letters (and changing their positions) and then asking someone to spell a certain letter or word which is also hidden under a covering cloth. The results, though suggestive, are not claimed to be conclusive.

The existence of automatic writing (i.e. of an intelligent activity carried on without the subject's knowing what he does) seems to be well born out by Bender's experimental findings. Some subjects were able to describe pictures by automatically spelling the respective sentences backwards while conversing with the experimenter on other topics—without knowing what they were spelling at any given moment, beyond one or two letters. This "simultaneous double-performance in a state of mental dissociation" seemed to excel over the achievements with conscious concentration. The previous literature is amply reviewed and discussed. The book is meant to inspire further studies rather than to "prove" anything.

Zum Problem der aussersinnlichen Wahrnehmung: Ein Beitrag zur Untersuchung des "räumlichen Hellsehens" mit Laboratoriumsmethoden. Von HANS BENDER. (Mit einem Geleitwort von E. R. JAENSCH.) Leipzig: J. A. Barth. 1936. Pp. xi+116.

In this second book Bender confines himself to the phenomenon of 'clairvoyance', i.e. extra-sensory perception of objective data which, at the time, are not known by any other human being (as contrasted with 'telepathy' which is the extra-sensory transfer of mental contents). The procedure was this: a series of cards with a letter printed on each was presented one by one to a subject, Frl. D., without the experimenter himself knowing the order, in two or three different ways; in none of these was there any possibility of visual or tactile perception of the object. The subject was asked to describe, draw and name what was on the card; this appeared to be projected like a hallucinatory or an eidetic image. The author puts more emphasis on the qualitative findings than on the quantitative ones; but the latter showed that the whole letter was rightly recognized in one-fifth to one-half of the cases in the various series. The author, for several reasons (see p. 91) does not regard this as sufficient evidence that the perceptual process was analogous to sensory perception, thus trying to keep in harmony with the findings of J. B. Rhine. A comparison between the gradual development of letters in clairvoyance and the gradual development of the same letters when exposed near the threshold of form-vision showed a marked similarity of phase. The author is led to believe that the clairvoyant projection of images has its origin in a dissociated, subconscious stratum of personality. This second book of Bender's, though far more conclusive than the first one, is still mainly suggestive. The presentation of the findings might have been better worked out.

Einführung in die Individualpsychologie. Von RUDOLF DREIKURS. (Mit einem Vorwort von ALFRED ADLER.) Leipzig: S. Hirzel. 1933.

El Libro del Saber. Por RAMON SOTO FERNANDEZ. Toledo: Associación de Huérfanos de Infantería. 1933. Pp. 150.

Logik und Wissenschaftslehre. Von J. KRUG und D. POMMER. Wien: Holder-Pichler-Tempsky A.G. 1936. S. 71. Mk 1.75.

The Psychological Methods of Word-Association and Reaction-Time as Tests of Deception. By H. R. CROSLAND. University of Oregon Publication: Psychology Series. 1929. Vol. I, No. 1. Pp. 104. $1.00.

Objective Measurements of Emotion. By H. R. CROSLAND and L. F. BECK. Same Series. Vol. I, No. 3. 1931. Pp. 202. $1.00.

The Effects of Eye-Dominance on "Range of Attention" Scores. By IRVING ANDERSON and H. R. CROSLAND. Same Series. Vol. IV, No. 4. 1933. Pp. 23. $0.25.

The Effects of Handedness on "Range of Attention" Scores. By IRVING ANDERSON and H. R. CROSLAND. Same Series. Vol. IV, No. 5. 1934. Pp. 16. $0.25.

The Effects of Combinations of Handedness and Eyedness on Letter-position "Range of Attention" Scores. By IRVING ANDERSON and H. R. CROSLAND. Same Series. Vol. IV, No. 7. 1934. Pp. 48. $0.25.

The Abnormal from Within: Dostoievsky. By S. STEPHENSON SMITH and A. ISOTOFF. Same Series. Vol. V, No. 2. 1935. $0.25.

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ELEVENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

IMPORTANT ANNOUNCEMENT

THE organizing Committee of the Eleventh International Congress of Psychology, in view of the coincidence of the date chosen for the Madrid Congress with the dates of meetings in various other countries, and in view also of the international situation, has felt obliged to consult the permanent International Committee as to the advisability of postponing the Congress to some future date. The reply being favourable, it was decided to postpone the meeting till the end of July 1937. The exact date will be announced later.

The meeting in Paris of the Second International Congress of Mental Hygiene in July 1937 and the meeting of the Ninth International Congress of Philosophy at the beginning of August will allow those attending these Congresses who are interested in psychological problems to attend also the Madrid Congress.

Preparations for the Congress are being carried on by the General Secretariat. Communications, reports, etc., are being received and classified in accordance with the schemes already drawn up, a more complete realization of which will be made possible by the postponement.

For all future arrangements it is requested that application should be made to: Dr José Germain, Secrétaire Général du Congrès, Instituto Nacional de Psicotecnia, Alberto Aguilera 25, Madrid, Spain.

INSTITUTE FOR THE STUDY OF ANIMAL BEHAVIOUR

THE Institute for the Study of Animal Behaviour has been formed by a number of zoologists, naturalists, physiologists and psychologists with the object of promoting and encouraging research into animal behaviour. The Institute intends to act as a clearing-house for information regarding work that is being done upon animal behaviour in all its aspects, and to bring together for the discussion of their problems field and laboratory workers. It is hoped to issue to members a quarterly bulletin in which will be provided summaries of and references to the chief work being done in the various branches of the subject. Meetings for the transaction of scientific business will be held at monthly intervals.

The Institute hopes eventually to obtain the means of supporting research.

Those whose work is concerned with animal behaviour will materially assist the task of the Institute if they will send offprints of their papers to the Hon. Secretary, R. C. Oldfield, M.A., The Psychological Laboratory, Cambridge.

BRITISH NATIONAL HUMAN HEREDITY COMMITTEE

THE British National Human Heredity Committee was formed in 1932 for the collection of data and the investigation of human pedigrees, as a branch of the International Human Heredity Committee, founded by the International Federation of Eugenic Organizations. It has now been enlarged as a Council and has secured accommodation at 115 Gower Street, London, W.C. 1, and in collaboration with the Galton Laboratory aims at setting up a Clearing House for material on Human Genetics. The Council consists of a number of geneticists and leading medical men, with Professor Ruggles Gates as Chairman, Sir Laurence Halsey as Treasurer, Dr Fraser Roberts as Honorary Scientific Secretary and Mrs C. B. S. Hodson as Honorary General Secretary. The direction of the work is to be in the hands of a small Executive Committee.

The Council would be grateful to receive all available material from institutions and individuals, furnishing well authenticated data on the transmission of human traits, whatever these may be. Family histories or pedigrees, twin studies and statistical researches are mainly contemplated. As research workers and others who send in material may in some cases wish to retain the sole right of publication or copyright, those who so desire are asked to accompany their material with a statement to that effect.

Reprints of published work would also be most acceptable. Many authors when publishing material may also have collected a number of pedigrees which they have been unable to reproduce in detail. It is the object of the Council that such records, by being included in the Clearing House, should not be lost. Those wishing a copy of the standard International Pedigree Symbols may obtain one from the Office.

Material should be accompanied by all available details in regard to source, diagnostic symptoms, and the name and address of the person or persons who vouch for accuracy. All such details will be regarded as strictly confidential.

The other objects contemplated in this enterprise, namely facilities for study, replies to enquiries, and information service, cannot be initiated for some time. Announcement of these activities will be made later.

VOLUME XXVII JANUARY, 1937 PART 3

THE VAGARIES OF AN EMANCIPATED PSYCHOLOGY

BY J. DREVER

RECENT developments in psychology in this and other countries make it abundantly clear that the present situation requires to be carefully considered in the light of the claim of psychology to take rank as a positive science alongside of the other biological sciences. That the emancipation, so to speak, of the science, and the expansion of its practical applications, have brought about a situation highly dangerous for the science itself there can be hardly any doubt. The designation 'academic' psychology, employed as a term of reproach, and even contempt, by the applied psychologist, when speaking of the pure, or, shall we say, scientific psychologist, is itself sufficient evidence of the danger. It is suggestive of an attitude on the part of applied psychologists towards scientific work in the psychological field, which, if allowed to pass without challenge, would very soon make psychology more of a byword among men of science than it ever was in its old metaphysical days.

By the emancipation of psychology I mean the attainment of freedom from the trammels of an orthodox doctrine, whether theological or philosophical, but particularly the latter. The attainment of this freedom has been a long and gradual process. Even yet, as far as the individual psychologist is concerned, the freedom is only relative, since an individual's attitude towards orthodox theological or philosophical doctrine almost inevitably affects his point of view in psychology. Nevertheless we all recognize that, so far as we profess to be psychologists, any bias deriving from such a source must be eliminated, and on the whole the elimination is fairly successful. It may be taken as granted that the development of psychology during the last half-century has been in the main on the lines of positive science, emancipated from the domination of either philosophy or theology. At the same time this very emancipation is to some extent responsible for exposing the science to influences which are making themselves felt at the present time as real dangers to its status and its progress.

It is of course essential that science should emancipate itself from theological and philosophical doctrines. The business of science is to observe accurately the various phenomena of the universe, in the first instance, and then to reduce these phenomena to some sort of order by formulating the laws in accordance with which they occur. The attempts at the formulation of such laws we speak of as 'theories'. Now theories have a certain fascination for the human mind, especially for some types of human mind. A theory, however, has no real value independently of the facts it summarizes, and a scientist who is attracted away from the observation of facts by the fascination of theories is in grave danger of very soon ceasing to be a scientist. He may of course avoid the danger. It is, however, in the relation of theories to facts that the logic of science comes in, and it is certain that many of the vagaries of pseudo-science arise from the neglect of the logic of science under the influence of the fascination of a theory. Here psychology has possibly lost something as a result of its emancipation from philosophy; in some cases it has emancipated itself from logic as well as philosophy. The philosopher-psychologist was usually fully awake to the possibility of fallacious reasoning. The reasoning which he knew and applied was, it is true, largely deductive, and the field in which it was applied relatively narrow, but many of the works in this field are models of close, acute, and rigorous reasoning. When the psychologist ceased to be also a philosopher something of this was lost, often without any compensatory gain.

In the new fields which the psychologist sought to conquer inductive reasoning—the characteristic reasoning of positive science—took the place of the deductive reasoning of the philosophical psychology. Moreover, the psychologist was faced with problems much more complex than any other positive science. The early experimental psychologists deserve great credit for their attempts to develop experimental methods adequate to their task—the psychophysical methods remain as a monument of these attempts. But these methods were also applicable only in an exceedingly narrow field. In the wider field psychologists without philosophical training, and often also without serious scientific training, have by no means been conscious to the same degree of scientific method. Not infrequently this has resulted in an ignoring of the laws of evidence under the influence of the fascination of a theory, and in the development, in the name of psychological theories, of conceptual structures which differ little, if at all, from the myths which satisfied the primitive mind in its first attempts to explain natural phenomena.

At this point another influence demands consideration. Myths satisfy the primitive mind because a logic has not yet developed. Human thought is, so to speak, at a pre-logical level, and this largely because reasoning, or the intellectual processes generally, have not disengaged themselves from the affective—feeling, emotion, sentiment. Now there will always be regression to this pre-logical level, wherever and whenever human thought reverts to this condition, which might be described as 'domination by affect'. Such regressions and reversions have been evident again and again in the development of human thought, whether as professed philosophy or as professed science. This is a second influence which we may expect to find underlying, in particular, vagaries of psychological theory. At the present time great attention is being given to these and similar phenomena in the individual mind by the psychoanalysts. This indeed is not the least of the services which psychoanalysis has rendered to psychology, since this whole field of the domination of thought by affect had been almost entirely neglected up to recent times by most representatives of the science.

At the same time it is well to bear in mind that the psychoanalyst has exemplified this very vagary of which we are speaking to a greater extent than the representatives of any other branch of psychology. It has frequently been suggested that those who come much in contact with paranoiacs tend themselves in time to exhibit paranoid symptoms. It is not unlikely then that those who are in constant contact with affect-dominated thinking will show the influence of affect in their own thinking, and in their own theories exhibit in consequence some reversion to a pre-logical level. That is at least one possible explanation of some of the unscientific characteristics of psychoanalytical theory which will be mentioned presently.

A recent writer[1] has called attention to another possible explanation of some of the unscientific characteristics of psychoanalytical theory, which is somewhat more general in its application. These characteristics, he points out, may be partly due to the fact that psychoanalysis is still in its infancy, but they are also due to what he calls 'the idols of the clinic'. The psychoanalyst is usually in the position of a physician, to whom the patient comes, believing that the physician understands his case, and can cure him. Unless the physician believes this also it is hardly likely that the patient can be got to believe it. "As long as they both believe, it is a matter of minor importance whether their beliefs are true." In consequence of this psychological situation "the art of healing", as the author in question says, "remains a hopeless tangle of sense and nonsense". There is of course no strong motive for observing the canons of scientific investigation. In consequence also of the fact that the psychological situation is of greater importance in the case of psychotherapy than in other branches of medicine, the influence of such 'idols of the clinic' is greater in the theories of the psychotherapist than in the theoretical foundations of any other branch of medical practice. Some 'idols of the clinic' may also have their origin in the fact that in the clinic the physician sees only the abnormal, and as a result he may tend to forget what normality is like. There is no sense in shutting our eyes to the urgent necessity of a minute and rigorous examination of all theories which have their basis in clinical observations. This is the kind of retort which the 'academic' psychologist is fully justified in making to the clinical psychologist.

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[1] Ritchie, A. D. (1936), *The Natural History of Mind.*

We may profitably consider for a moment psychoanalytical theory as illustrative of the manner in which various influences combine to produce what we may call pseudo-science. There are other reasons also for such consideration. It is frequently stated by more or less orthodox psychologists that it is easy to criticize destructively the theories of Freud and his school. They generally leave it at that, however, without attempting detailed criticism. For the sake of the science of psychology it is highly desirable that the main points of criticism should be definitely formulated, so that there should be no doubt whatever as to where scientific psychology stands. It ought to be possible to do this while at the same time making full acknowledgement of the real and important services rendered to psychology by Freud and the psychoanalysts, indeed by clinical psychologists at large.

All scientific theories are based on the direct observation of phenomena. Their validity will therefore depend, in the first instance, on the adequacy of the observations. Apart from this their validity will also depend on the validity of the mental operations involved in the passage from the concrete observations to the theory. From both these points of view the validity of many of the psychoanalytic theories—if we admit their status as scientific theories, rather than myths or parables—can be challenged.

Consider first the observation aspect. In the case of psychoanalytical theories the observations upon which they are often based are, from any rigorous scientific point of view, open to the most damaging criticism. In the first place the observations are made under conditions which are inadequately analysed or controlled. In the second place the subjects are for the most part in a highly abnormal state, often in part deliberately induced by the psychoanalyst. In the third place no complete contemporary record of what actually transpires is available, the record kept usually depending on subsequent recall by the analyst, a recall that can hardly ever be free from the influence of the analyst's theories, by way of omission or insertion. In the fourth place there is no possibility of verification by repetition of the observations under precisely similar conditions, since the analysis has already permanently changed the conditions.

The validity of a theory cannot be independent of the facts upon which it is based, and if there are no facts, or only facts which have been generally and vaguely ascertained, it is obvious that the validity and scientific value of the theory must be regarded as highly doubtful. Add to this the consideration that many of the facts alleged by psychoanalysts, in support of their theories, are themselves already theories or interpretations of facts, which facts are seldom or never forthcoming. In other words the psychoanalyst alleges that he has observed such and such, usually not the exact observation made, but an interpretation of it, and we ask in vain for any further evidence. We neither know what was observed, nor the conditions under which it was observed. We are expected to accept the *ipse dixit* of the psychoanalyst. This is not science as the modern world understands it. It is of the very first importance that we keep these things before the mind when considering the scientific validity of psychoanalytical theories. There are perhaps other branches of psychology in which similar strictures apply, but in none to such an extent.

Similar strictures apply to the manner in which psychoanalytical theory has been, and is being, built up. In his little *History of Psychology*, J. M. Baldwin characterizes the early pre-logical gropings of the primitive mind towards some sort of account of mental life as 'psychosophy', and he points out that, not only did such 'psychosophy' precede the science of psychology, but it has also always remained a thorn in the side of the science. He goes on: "Socially established superstitions, occult rites, mystic appearances, religious wonders, animistic and spiritistic realities, systems of 'new thought' and 'Christian Science'—these do not make appeal to logic, or recognize the demand for objective proof. They rest in collective representation: or they are sanctioned by tradition; or they represent types of affective value; or they make appeal to emotional and gregarious habits of mind. In short they represent and find their refuge in practical interests, in behalf of which they continue to scout the claims of the theoretical." Judged by the writings and utterances of some psychoanalysts, psychoanalytical theory seems to have an ambition to become a 'psychosophy'; it undoubtedly has many of the marks. If guessing, or shall we say, intuition and phantasy, are to be substituted for patient observation, careful analysis, and rigorous logical procedure, in developing the various theories or myths, then that is certainly what it is going to become.

It is our bounden duty to get a fair estimate of the value of the theoretical side of psychoanalysis, and the only point of view from which this appears to be possible is when we regard the various theories as merely descriptive, rather than explanatory or interpretative in a scientific sense—as picturesque myths, many of them ugly enough, but all of them interesting, myths which represent symbolically what appears to take place in the affective and impulsive life of the human being. When we regard them in this way we can, of course, relax the demand for the rigorous logical procedure of positive science. Affective and impulsive phenomena are entirely alogical; a description of them in metaphorical or symbolical terms may also be alogical. But it must be clearly realized what the description is. When such a description professes to be, not merely an exact and literal description, but even a scientific explanation and interpretation, objection may quite rightly be taken to it. 'Projection' and 'introjection' and 'ambivalence' and a host of similar descriptive terms, useful though they may be, add nothing to our understanding of the phenomena; they merely give them names.

Some defenders of psychoanalytical theory have argued that, because the phenomena studied are alogical, the theory must also be alogical. That appears a really extraordinary line of defence. In effect it involves a denial of the principles of science, or alternatively the assumption that no science of these phenomena is possible. It suggests indeed that psychoanalytic theories are on the same footing as the phenomena they profess to interpret, and that they may therefore themselves be regarded as matter for analysis, with the object of throwing light on the mental life of the analysts, rather than as serious scientific theories. The psychologist cannot of course deny that his science owes a deep debt to Freud and his followers, but the debt is because of the new and important lines of investigation which they have opened up. For the rest, investigation is only at its beginning. Even alleged observations require careful examination, and most of the theories cannot as yet be given rank as scientific theories at all.

I do not wish to leave the impression that the vagaries of present-day psychology are confined to the psychoanalytical development. I have devoted so much attention to psychoanalytical theories because they probably afford the best illustrations of what can occur when the principles of science are ignored. The extreme behaviouristic development, however, notably in the shape given it by Pavlov and his followers, illustrates the same kind of vagary, though in a somewhat different way, both with respect to the observation of phenomena, and with respect to the formulation of theories. The 'idols of the laboratory', as Ritchie puts it, here take the place of the 'idols of the clinic'. No facts are taken into account except those amenable to study by a very narrow laboratory technique. That technique, admirable of its kind though it may be, is precisely such as to exclude the possibility of observing many kinds of psychological phenomena, often clearly relevant to our problem. "A model of experimental technique", says Ritchie of Pavlov's work on dogs, "from the point of view of eliminating uncontrolled variables; unfortunately the dog was one of the variables eliminated."[1] Again we have inadequate observation, though in a different way. Moreover, the theories go far beyond the observed facts, and ignore altogether the limitations under which the facts have been observed. That kind of thing is far too common in psychology to-day. It is the 'fascination of the theory' once more. Psychoanalysts are not alone in yielding to that fascination; nor indeed are behaviourists their only fellow-sinners.

To sum up: The vagaries of present-day psychology in its professedly scientific developments are largely the outcome of its recent history. The four main influences underlying them are: (1) the 'idols of the clinic', (2) the 'idols of the laboratory', (3) the fascination of a theory, and (4) the domination of affect. I think it is well that the psychologist should be on his guard against all four. The last is the most serious, and involves the very negation of science. The only hope of a real science of psychology is in the careful ascertainment of facts in the first instance. In the meantime theory can wait.

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[1] *Op. cit.* p. 105.

(*Manuscript received May* 24, 1936)

AN INVESTIGATION OF THE RELATIONSHIP BETWEEN THE PROCESSES OF REPRODUCING AND RECOGNIZING SIMPLE FIGURES, WITH SPECIAL REFERENCE TO KOFFKA'S TRACE THEORY

By O. L. ZANGWILL

(*From the Cambridge Psychological Laboratory*)

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I. OBJECTS AND PROCEDURE

(1) *Nature of the investigation*

THE successive reproduction of presented drawings after varying intervals of time has been frequently utilized as a method for the study of imagery and memory 'traces', and in particular of the changes and modifications which the latter are supposed to undergo with lapse of time.

Titchener (8) referred to such experiments as being 'instructive', but reported no systematic experimentation. Later workers in this field include Wulf (9), Gibson (4), Allport (1) and Perkins (7). Although the general procedure of all these investigators was essentially similar, there has been considerable discussion as to the types of material considered suitable, and the conditions regarded as 'valid' in such experimentation. In fact, the question of experimental method in studying this particular problem has become acutely significant in view of the radical differences of opinion and interpretation manifested by adherents of opposing schools.

With the possible exception of Claparède (3), no investigator has, to the knowledge of the present writer, attempted to assess the bearing of unwitting transformations and deviations in reproductions of a presented standard figure upon the nature and accuracy of subsequent recognition of this same figure. Koffka (6), however, emphasizes the importance of studying this relationship, and affirms that he hopes shortly to produce relevant evidence.

(2) *Objects of this investigation*

(1) To compare reproductions of a standard figure, obtained after prescribed intervals of varying length, with that figure identified as the standard, when the original figure is subsequently re-presented, together with other figures deviating from the former in varying degree, and particularly in such directions as are observed to obtain in the subject's own reproductions.

(2) To investigate whether observed phenomena of recognition are consonant with Koffka's recently elaborated Trace Theory, and to attempt to assess the degree to which recognition may or may not be influenced by such changes as are believed by Koffka to occur in his postulated trace systems.

(3) *Experimental method*

A standard figure is exposed for a prescribed interval. After varying lapses of time, reproductions are obtained. Copies of these reproductions, or figures based upon them, are introduced into a series, the remaining members of which comprise the standard, and certain other figures, whose nature and construction shall depend upon the types and trends of change observed in the preceding series of reproductions. The subject is then called upon to recognize what he believes to be the original standard figure. This procedure was carried out under strictly experimental conditions but employing only three subjects and ten standard figures. All conclusions must hence be regarded as essentially tentative, and based on results which make no claim to be adequate—far less comprehensive. Nevertheless it is fair to maintain that the results of the experiment are in themselves valid, for although no generalizations can be made from them, we are enabled to test whether or no theories purporting to be general, do, in effect, satisfactorily account for our results.

(4) *Subjects*

Three adult subjects co-operated in this investigation. Subjects A and B are research students in economics; A has had a mathematical training, whilst subject C is a third-year medical student. The subjects were ignorant of the object of the experiment and had read no 'Gestalt' psychology. Each was tested individually and no collaboration was permitted at any stage of the experiment.

(5) *Standard figures*

Ten drawings were adopted as the standards. With one exception all have been devised by other authors for experiments bearing on remembering, and would therefore seem particularly appropriate for this investigation.

Each figure is drawn in black ink on a card of dimensions 4 by 3 in., but the sizes of the individual figures vary slightly with relation to one another. In view of certain objections raised against Gibson on account of the small size of his diagrams, all figures employed which have been adapted from his material have been enlarged.

(6) *Apparatus*

(*a*) *For original exposure.* The apparatus comprises only a horizontal sliding shutter in a wooden frame. The shutter is operated by hand. Each card can be supported vertically in a horizontal groove running behind the shutter. Time control is by stop-watch.

(*b*) *For recognition.* The cards are exposed in a row simultaneously, and held in position by insertion in a groove along the upper surface of a long strip of wood.

(7) *Procedure*

(A) *Conditions of presentation.*

(*a*) *Time of exposure.* Five seconds for each standard card.

(*b*) *Mode of presentation.* The ten standard cards are presented in five pairs (*A*, *B*, *C*, *D*, *E*). The interval between the beginning of exposure of each of the two members of one pair is in every case 1 min.

The first and second pairs (*A* and *B*) are exposed on the first day, the third and fourth (*C* and *D*) on the third day, and the final pair (*E*) on the tenth day of the experiment.

(*c*) *Instructions.* The only instructions given were the following: "Please observe carefully the two figures which will be successively exposed. Each exposure will commence on the word 'Now'."

(B) *Conditions of reproduction.*

(*a*) *Time intervals.* Reproductions were obtained from the subjects after definite periods of time had elapsed since the original exposure. The arrangement of these intervals was as follows:

Pair *A*: 30 sec.
Pair *B*: 30 sec., 1 day.
Pair *C*: 30 sec., 2 days, 4 days.
Pair *D*: 30 sec., 2 days, 4 days, 6 days.
Pair *E*: 30 sec., 2 days, 5 days, 10 days, 18 days.

Although the exact time at which every reproduction was begun was carefully noted, it seems sufficient for the purposes of this investigation to record only the approximate duration of each interval.

(*b*) *Instructions.* For every set of reproductions, the subjects were each given a sheet of paper, at the top of which were the following instructions: "Please reproduce the two drawings which you just saw as accurately as you can." (Initial reproductions.) "Please reproduce the two (four) drawings which you saw on (date of exposure) as accurately as you can."

After reproducing the drawings, the subjects were in every case requested to write an introspective report of their mental processes during the period of reproducing the two figures. In the case of the first pair (*A*), the subjects were ignorant of the fact that they would later be requested to retrospect, but in the case of subsequent reproductions, they naturally suspected it.

(C) *Conditions of recognizing.*

(*a*) *Material and procedure.* In the case of the first pair (series *A*) the standard figure was re-presented together with five other figures, deviating from the standard in such directions as variation in size, disproportional increase or reduction of certain attributes relative to the remainder of the figure, and increase or decrease in structural complexity. In all remaining cases, the standard was re-presented together with from five to seven other figures, some of which are copies of all, or some, of the subject's own reproductions. In the case of geometrical figures, the copies were modified so as not to be immediately recognizable as freehand drawings. In such instances the size and proportions of the diagram were identical with the size and proportions of the reproduced figures, but lines intended by the subject to be straight were ruled as straight, and segments of circles were likewise accurately constructed.

Where progressive changes were observed in a given series of reproductions, one or more figures were included in the corresponding recognition series, which exhibited one or more of such deviations in a more advanced stage than was manifested in the final reproduction. As a rule, one or two members of each recognition series were figures differing from the standard in directions utterly divergent from those observed to occur in the subject's reproductions.

It will be seen that a special recognition series had to be constructed for each individual subject.

Between the presentation of the recognition series corresponding to each member of a given pair of figures, the interval was, as a rule, 2 min. In some cases, however, it was 1 min., and, for the last series, somewhat longer. The subjects were not restricted by a time-limit.

(*b*) *Instructions.* Before presentation of each recognition series, the subjects were shown a card bearing the following instructions: "You will be shown a row of drawings, one of which you have already seen. You are requested to identify the latter. Each drawing is numbered, and please write 'Yes' on the paper provided against the number corresponding to the drawing you recognize.

"If you cannot identify any diagram with certainty, please write 'probably' against the number corresponding to the diagram about which you feel the most positive. If you do not recognize any diagram, please leave the paper blank."

(*c*) *Time intervals.* The recognition series corresponding to the first pair was presented 5 min. after the termination of exposure of the second member. In the case of the remaining four pairs, the appropriate recognition series was presented in each case on the day following that on which the final reproductions had been obtained.

II. RESULTS AND DISCUSSION OF THE PROCESS OF REPRODUCING

(1) *Size of reproductions*

Exact reproduction of size, even in the initial reproductions, turns out to be very rare, and frequently later reproductions are more accurate than are earlier drawings in this respect. Successive reproductions exhibit no constancy in size, but on the other hand no satisfactory evidence for the regular occurrence of progressive changes in size in any given direction was obtained. In some cases, all reproductions tend to be consistently larger or consistently smaller than the standard, but such regularity is by no means universal. One or two series exhibit a certain periodicity in size, successive reproductions being alternately large and small.

Allport (1), employing 350 subjects and obtaining his final series of reproductions after a lapse of four months, found a striking reduction in size in the final drawings as compared with the originals. Koffka (6), discussing these results, suggests that within given ranges of size, definite tendencies towards either expansion or reduction may occur, which he attributes to certain autonomous processes in dynamic traces. Though evidence in this research is totally insufficient to constitute the experimental test demanded by Koffka for the resolution of this problem, it is nevertheless a fact that the material obtained under the conditions employed in this investigation does not exhibit any indication of progressive changes in size akin to those reported by Allport, who was working under but slightly different conditions. This anomaly must await interpretation.

(2) '*Figure-assimilation*'

No instances of 'Figure-assimilation', in the sense defined by Gibson (4), occurred in this investigation. All standard figures employed were quite specific, in the sense that no particular one was likely to be apprehended by the observer as in some respect similar to one or more of the others. Each series, moreover, comprised but two figures, often contrasting sharply, and purposely selected so as to reduce the possibilities of 'figure-assimilation' to a minimum. Furthermore, the observer had at no time expressly to 'remember' more than four standard figures.

(3) *Progressive trends of change*

(*a*) *Views of Wulf and Koffka.* Wulf (9) believes that the final reproduction in each series of his experiment constitutes a "more characteristically and distinctively pregnant structure" than the initial reproduction. According to him, deviations from the standard are always in determinate directions, which are, as a rule, indicated clearly in the first reproduction, and which may be interpreted in terms of 'sharpening' and 'levelling'. Wulf considers that such trends of change result through the agency of three distinct causal factors: normalization, emphasizing or pointing, and autonomous change. This view is endorsed by Koffka (6), who interprets Wulf's conclusions in terms of a dynamic trace theory.

(*b*) *Relation of our results to the above views.* In spite of an intensive analysis of the material, the difficulty in interpreting all deviations from the standard in terms solely of sharpening and levelling turns out to be very considerable, and such a procedure cannot be regarded as wholly free from artificiality. Furthermore, the inference of relevant causal factors would appear at best to be very hazardous. Even in the first series of all, where the deviations exhibited are few and regular and might readily be classified as 'sharpenings' or 'levellings', it proves almost impossible to infer with any degree of certainty whether the changes should be referred to the influence of pointing, normalization, or autonomous change operating in isolation, or to a combination of these tendencies. For instance, in cases where the 'spike' *CDE* of figure A (1) (see Fig. 1) is disproportionately reduced relative to the 'rectangle', the change may have been brought about by pointing, on account of dominance of the 'rectangle' in the perceived pattern, by normalization, or simply by autonomous changes in the direction of greater symmetry, stability and pregnancy.

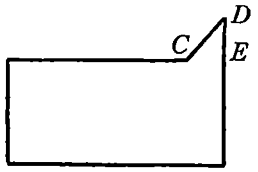


Fig. 1. Standard figure A (1)

Rigid analysis of the deviations in terms of a preconceived theory is, for the more complex figures, well nigh impossible. These changes were nevertheless analysed as far as possible in terms of Wulf's categories. The following results were obtained:

(1) In series *A* the total of distinct deviations occurring in the reproductions of all subjects in the direction of reciprocal sharpening and levelling of the principal structural aspects of the figure amounted to seven.

(2) Changes which appeared in the first reproductions of pairs *B*, *C*, *D*, *E*, and which are either maintained relatively unaltered throughout the entire series of reproductions, or else form progressive trends of deviation in the direction of reciprocal sharpening and levelling of the principal structural aspects of the figure, are termed *Persistent Deviations*.

These may be conveniently subdivided into two classes, comprising the *Stereotyped* and the *Continuous* types of deviation respectively.

The total of Stereotyped and of Continuous Deviations for all subjects amounted to twenty-eight and twenty-six respectively. These results imply that of the total Persistent Deviations tabulated, 52 per cent are of the Stereotyped and 48 per cent of the Continuous variety.

(c) *'Object-assimilation', normalization and conventionalization.* Wulf and Gibson, in spite of divergent definition and interpretation, both call attention to the fact that reproduction of a given standard figure may in some cases progressively deviate from the standard in the direction of a familiar object.

This change, though remaining, according to Wulf, logically a sharpening or a levelling, constitutes the normalization of Gestalt Psychologists, and is explained by Koffka as a result of interaction either between a new trace and its corresponding class trace system, or in some cases between this trace and 'outside' trace systems.

Gibson terms this type of change 'object-assimilation' and defines it as "the phenomenon in which the perception of the figure involves visual or verbal imagery of some familiar object or shape, and where the reproduction which follows is clearly changed so that it more nearly resembles the familiar thing than does the stimulus figure". The relation of these alleged processes to the process described by Bartlett(2) as 'conventionalization' is obviously a very close one, and the trend of change manifested for instance by Subject A in series E (2) could probably be satisfactorily interpreted from all three points of view. 'Conventionalization' appears to the present investigator by far the least compromising term with which to denote this type of process, for there is on the one hand no unequivocal evidence that "visual or verbal imagery of some familiar object" must *necessarily* occur in order for reproduction to approximate continuously towards the object in question, and on the other hand employment of the term 'normalization' as interpreted by Koffka involves implicit acceptance of his speculative trace theory.

In considering the results of this investigation, many of the changes classified as 'autonomous changes' may very possibly constitute 'normalizations' or 'conventionalizations', and although such a series of reproductions as E (2) of Subject A furnishes an excellent example of conventionalization, it is believed that the data are insufficient for a crucial analysis of this problem.

(d) *Conclusions.* (1) Many of the deviations occurring in the reproductions may, although not always without risk of artificiality, be interpreted as a 'sharpening' or a 'levelling' of some attribute or aspect of the figure with reference to the structure and proportions of the whole.

(2) In some cases such deviations are manifested in the first reproduction, and thereafter re-occur without further significant change. These we term 'Stereotyped Deviations', and they comprise 52 per cent of the total of Persistent Deviations recorded.

(3) In many cases, however, continuous trends of deviation do occur, which may in most cases be interpreted as a progressive 'sharpening' or 'levelling' of some portions of the figure with reference to the corresponding proportions in the standard. These we term 'Continuous Deviations' and they amount to 48 per cent of the total of Persistent Deviations. In some cases these changes are in the direction of the attainment of greater symmetry by the pattern as a whole, but if we accept the validity of the Gestalt view, an explanation of trends of deviation in the majority of cases must necessitate some such assumption as that for a given individual, a given type of symmetrical figure may possess greater stability, simplicity and 'pregnancy' than an objectively closely allied type of symmetrical figure.

(4) It is very difficult positively to ascribe a given trend of change to the agency of a single causal factor. Many deviations may, with equal theoretical justification, be interpreted as due to normalizing, to pointing, to autonomous change, or to a combination of these three influences.

(5) Certain types of continuous trend have been termed 'normalization', 'object-assimilation' and 'conventionalization'. Though differently defined, it looks as if all these terms refer to a single type of process, for which Bartlett's term, conventionalization, is by far the least ambiguous.

(4) *Stereotyping of successive reproductions*

It has already been pointed out that many deviations from the standard which are not continuous are found to occur in the first reproduction. Successive reproductions in these instances resemble one another very closely. Later reproductions deviate from the standard little, if any, more pronouncedly than did the earlier. There are several possible lines of interpretation. It is possible that the most 'characteristically pregnant' structure is achieved immediately through this standard degree of deviation; in which case, of course, further progressive change could not be expected to occur. It is also conceivable that additional factors come into play which mask or counteract the postulated continuous tendencies, or even that Wulf's conclusions are not valid under all conditions. Nevertheless, any hypothesis that is advanced to account for the progressiveness of deviations under certain conditions should also be capable of explaining the stereotype in reproduction that undoubtedly may obtain under apparently similar conditions.

An important influence is undoubtedly exercised by verbal formulations and analysis. This is considered in detail in the following section. All subjects frequently analysed the figures in verbal and geometrical terms, and drew them again and again according to a resulting formula, which was itself memorized and recalled. As one subject spontaneously reported: "I get the proportions of the figure worked out and the actual drawing stamps them in." The occurrence of stereotyped successive reproductions is but one instance of a general outcome of this investigation, which is an express emphasis upon the necessity for great caution in inferring any general parallelism between changes occurring in overt reproductions and in hypothetical memory traces.

(5) *Verbal analysis and the constructive character of reproducing*

"Change due to verbal analysis", writes Gibson, "...occurs when the stimulus form...is analysed verbally in any variety of ways, and when the reproduction is so changed that it is wholly or partly a product of the verbal analysis rather than a representation of the form itself." Koffka believes that this type of change is due primarily to the fact that the subjects were obliged to use a 'special device' to memorize these figures against 'the destructive forces of aggregation', and that this phenomenon was largely incumbent upon Gibson's mode of presentation of his material (Ranschberg memory apparatus). Partially justified though this criticism may be, verbal analysis and kindred processes seem far more habitual and essential in remembering than Koffka appears willing to admit. In the present investigation, each series comprised but two figures, and each figure was independently exposed for 5 sec.; nevertheless verbal analysis has, according to the introspective reports, played a very considerable part in the process of reproducing, and in some instances seems to be very largely responsible for resulting deviations from the standard. Even in the earliest stage of the experiment, when the subjects were aware neither that they would subsequently be required to reproduce the figures, nor having reproduced them, to retrospect, the influence of verbal analysis can be clearly traced.

Bartlett's work has unequivocably established the fact that normal remembering typically displays an essentially constructive or reconstructive character. Much of the material from this investigation supports his conclusion, and were it not for the fact that progressive trends of deviation in the direction of certain types of symmetrical structure do undoubtedly occur, one would be tempted to interpret every process of reproducing as essentially a reconstructive process.

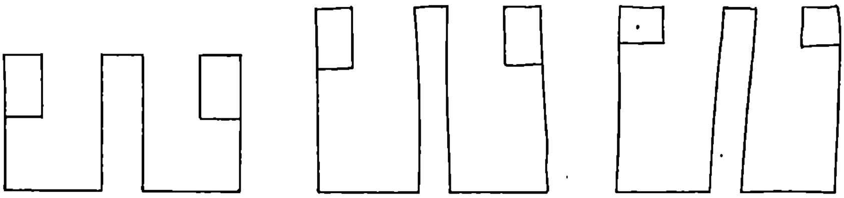
For the reconstructive nature of such reproduction, we are in possession of three distinct lines of evidence:

(1) That derived from an analysis of the introspective reports;

(2) That derived from studies of non-progressive (stereotyped) and inconsistent deviations from the standard;

(3) The difficulty in classifying all the types and trends of modification observed within the rigid framework of the Gestalt theory.

(1) *Evidence from the analysis of the introspective reports.* When requested to reproduce any figure, Subject A as a rule literally constructed the figure according to certain verbal and symbolic formulae which he had evolved to express the proportional and structural relations. Subject C, too, in many cases employed a similar method, and again and again mentions the terms 'construct' and 'reconstruct' in his introspective report: e.g. "I constructed the figure from these dimensions." "It was difficult to reconstruct the angles of the two sets of lines." Subject B analysed his figures considerably less in purely geometrical terms, but many of his introspections reveal well the constructive character of reproducing: e.g. "I simply reproduced the figures as I remembered them, imagining the first to be a rectangle with a slight projection at the corner, the second as an inverted 'u' with appendages at either end." In this subject's *E* (1) series, his introspective report reveals great uncertainty as to whether the 'rectangles' were in fact rectangles or squares. In his reproductions, they are drawn sometimes in one of these ways, sometimes in the other (see Fig. 2).



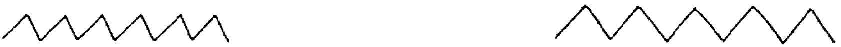
Standard figure *E* (1). B's second reproduction. B's third reproduction.

Fig. 2.

In some cases, particularly in series *E* (2), construction is so marked as to constitute importation and invention, and may, in some cases, be explicitly admitted, as for instance: "The arrangement of the name is guesswork", from Subject C.

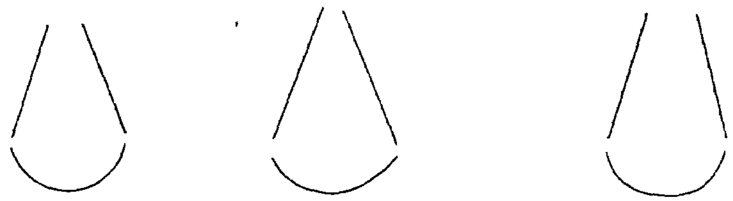
(2) *Evidence from non-progressive and inconsistent deviations.* It must be remembered that 'sharpening' and 'levelling' are essentially logical distinctions, analytically derived categories of change. The fact that a deviation can theoretically be classified in this manner does not legalize the inference that the change must necessarily result from the interplay of forces within and between strictly localized trace-systems.

Many instances of deviations did, however, occur which could be analysed in terms of sharpening and levelling. For instance the disappearance of asymmetry in *C* (1), the accentuation or reduction in the width of the gaps in *B* (1) and *D* (1) (see Fig. 3), and the alterations in curvature of arcs of circles, would all seem to be cases corroborating the Gestalt theory.



Standard figure *C* (1).

B's first reproduction.



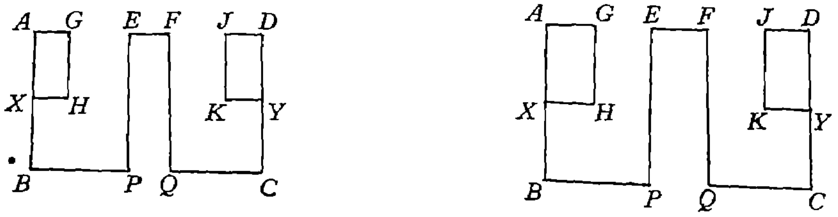
Standard figure *D* (1).

B's third reproduction.

A's second reproduction.

Fig. 3.

On the other hand, many other instances of sharpening and levelling occur which do not admit of such ready interpretation. Thus in series *E* (1) (see Fig. 4), Subject C has 'levelled' the figure to the extent of drawing $AX=BX$ and $DY=CY$, but sharpened the distance EF such that $GE=EF=FJ$. He states in the accompanying report that he noted that these dimensions were as far as he could see equal, and that DJ was a bit shorter than FJ, about in the proportion that FJ was shorter than DY. He writes that the figure "was constructed from these dimensions".



Standard figure *E* (1).

C's first reproduction.

Fig. 4.

These observations do not, of course, confute any principle in the Gestalt theory; perhaps the figure in question was perceived in the way described because for this particular observer, a pattern of this particular structure constituted a 'better', i.e. a more 'pregnant', pattern than did the original.

Such observations do, however, indicate the enormous complexity of perceptual and mnemic processes and the danger of inferring causal factors from reproductions unqualified by introspection. In the instance considered above, moreover, the fact that subsequent reproductions appeared completely stereotyped, each being constructed on the basis of the dimensional scheme initially evolved, demonstrates that autonomous changes, if they really occur, may either be masked in some way, or else undergo compensation by other factors. Such compensation may, perhaps, be brought about by reproduction being realized through other methods, or, in Koffka's terminology, through the agency of 'outside' trace-systems. It remains possible, of course, to regard verbal analysis itself as a type of conventionalization, and deviations in the reproductions resulting from such processes of verbal formulation may, with a certain justification, be therefore regarded as the outcome of a particular and complicated type of normalization. Though this latter interpretation may be theoretically legitimate, its acceptance would render any explanation in terms of interaction within trace-systems so cumbrous a procedure as seriously to endanger both the validity and the utility of such an approach.

(3) *Evidence from inadequacy of the Gestalt theory.* We have shown that the mental processes involved in reproducing even the simplest figures are by no means themselves simple. Elaborate verbal and symbolic formulations, naming and 'object-assimilation', visual images and vague feelings of accuracy and inaccuracy, all appear operative in determining the structure of the figure reproduced. With more complex material, changes such as the following occur: translations of words into other languages, reduplication of detail, and conventionalization, all of which demonstrate even more forcibly the constructive character of remembering.

To attempt to clarify such processes in terms of a system of hypothetical neuro-dynamics appears to the present investigator premature, to say the least, and in order to render a comprehensive account of the reproductive process even in the case of drawing objectively simple figures, it would be necessary to elaborate a quasi-physiological system of overwhelming complexity. It is surely far more profitable to attempt an analysis of the conditions under which these various related processes operate, and to attempt to determine their relative significance in remembering in terms both of types of material and individual peculiarities.

(6) *Validity of the criterion*

It is usually assumed that deviations in successive reproductions of a simple figure from the original constitute an index or criterion of corresponding changes occurring in memory images, in traces, or in both.

The view that deviations in reproductions from the original figure mirror deviations in the memory image from the original perception seems to have been held by Titchener and, with some reservations, by Allport. The latter introduces his study of successive reproduction as an "examination of the characteristic lines of change and decay in the visual memory images of children". Although our investigation was unfortunately carried out on a much smaller scale than that of Allport, it seems to show conclusively that in some individuals at least the reproduction of simple figures is by no means a simple function exclusively conditioned by visual imagery, as was indeed shown long ago by Wulf, and that great caution must be exercised in referring changes in reproductions back to strictly corresponding alterations in images. Since Allport did not collect introspections, his study must be regarded exclusively as a study of the 'lines of change and decay' in the *successive reproductions* of children. Wulf, who obtained introspective material, interprets his data with much greater caution and thoroughness. He concludes that visual imagery did not exclusively determine the reproductions in even 50 per cent of the cases, but brings forward evidence that changes in the direction of sharpening and levelling are manifested not only by the drawings, but also, and in a more pronounced form, by the image. All processes involved in remembering are thus held to be governed by the same law (Pregnancy).

Koffka generalizes further, and utilizes changes in reproduction as valid criteria of changes in traces. Perkins, on the other hand, employs his results as material for the development of a theory which repudiates the concept of specific traces. Gibson attempts the interpretation of a closely similar experiment in terms of 'perceptual habits'. Two factors emphasize the great necessity for extreme discretion in drawing inferences from the outcome of this type of experiment: The first, the wealth of divergent interpretation that can result; the second, the apparent complexity of the reproductive process even when operating with simple material. The present writer is convinced that a far truer picture of the processes involved in remembering is obtained through intensive analysis of the data in terms both of individual subject and individual figure than through the elaboration of rigid 'Theories of Memory' involving questionable inference and statistical schematization.

III. RESULTS AND DISCUSSION OF THE PROCESS OF RECOGNIZING

(1) *Theories of recognition*

Having analysed the types of change exhibited by the reproductions, it is now proposed to examine the possible relationship between such changes and accuracy in recognition of the originals. The connexion is, no doubt, a very indirect one, but the evidence promises to throw some new light on the relation between remembering (recall) and recognizing.

It is not proposed to catalogue the theories of recognition; they have been discussed in detail by many authors, particularly Katzaroff(5), Bartlett and Koffka. Bartlett has, however, shown that the familiar theories of recognition need not be regarded as mutually exclusive, and indeed may be considered as descriptions of the "varieties of mechanism through which recognition may work". He has shown further that recognition depends on "the coincident arousal of two different functions: (1) a specific sensory reaction and (2) an attitude or orientation", and makes it abundantly clear that there can be no recognition without both, though on the other hand, when both are present, it cannot be concluded that recognition is bound to follow. In general, recognizing is possible if the orientation or attitude which marked an original perception is carried over to re-presentation.

Koffka's approach is much more speculative, and very difficult to summarize. The following is a very incomplete account of his theory, which cannot presume to do justice to the originality of his views: Recognition is envisaged as a communication between process and trace. The particular trace corresponding to the prior perception of the object now represented is selected by the present process, not causally because it is similar, but because this particular communication would lead both to an improved process and to a condition of greater stability of the trace. Koffka links up his theory with that of Claparède by postulating an Ego-part and an object-part of the trace. "The closer the dynamic intercourse between the Ego and the object-part, the more likely, *ceteris paribus*, will recognition be." On re-presentation of an object, the process corresponding to its perception by the Ego communicates with the retained Ego-object trace by virtue of the principle outlined above. The quality of familiarity is explained thus: "When we see the well-known object in its normal environment, then our attitude towards it is also, as a rule, the normal one. Therefore, the process which communicates with the trace is not simply the process of perceiving the object, but the process of perceiving this object, which, corresponding to the prevailing attitude, is in a definite relation to the Ego. This new process will at once connect with the whole Ego-object trace; conversely, when the object is encountered in a new environment with a different Ego-attitude, then the communication takes place between object-process and object-trace, and the effect of this communication of a part-trace with a part-process will be the one derived in our theory of reproduction: the whole trace will be put under tension. We derived from this tension the possibility of spontaneous reproduction, and indeed recognition is very often accompanied by spontaneous reproduction. But we are now forced to the conclusion that this stress, if it involves the Ego-object pattern of the trace, must also account for the quality of familiarity in recognition." Koffka does not, however, confine recognition to cases where this quality is felt, for a communication between process and trace may occur and be of a kind which produces no stresses in the trace.

The results of this investigation will be discussed from the point of view of both these theories.

(2) *Results of this investigation*

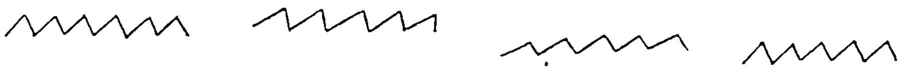
The unit selected for assessing the relationship between deviations manifested in reproductions and by the figure recognized is the *Persistent Deviation*. This is defined as a deviation from the standard, which can be regarded, at any rate conceptually, as a change in the direction of either sharpening or levelling with regard to the proportional relations of the standard figure as a whole, and which is maintained throughout a given series of reproductions of a given subject. As has been already pointed out, Persistent Deviations (P.D.'s) fall into two classes: (*a*) Progressive or Continuous Deviations (C.D.'s) and (*b*) Stereotyped Deviations (S.D.'s).

The significance of these terms has been explained above, and it must be understood that their use involves no theoretical presupposition. Many stereotyped deviations may represent the rapid attainment of a 'dynamically normal stability', such that the continuous nature of the change would not be apparent. On the other hand, both classes of deviation could probably be interpreted on theories other than those of the Gestalt school.

The whole procedure involved in this analysis may be regarded as somewhat artificial. Thus it may be objected that the unit employed should not be a single characteristic of a figure, but the whole of the figure. Though such a method would seem, no doubt, more satisfactory, the practical difficulties that would then supervene in the face of any attempt towards a quantitative evaluation of results would appear insuperable. For instance, two specific and distinct Persistent Deviations may occur in a series of reproductions of a given subject. The figure identified by him as being the standard may exhibit one parallel deviation but not the other. Hence a positive relationship may be said to obtain between one P.D. and a given character of the figure recognized, but a negative relationship between the other P.D. and the second character of the figure recognized, which latter, in so far as it concerns this second character, is identical with the standard.

As an actual example of this, we may instance series *C* (1) of Subject C (see Fig. 5).

His reproductions furnish two examples of Persistent Deviations of the stereotyped variety (S.D.'s); e.g. (*a*) omission of one element of the zigzag pattern, and (*b*) an exaggeration of the original asymmetry. The figure that he recognizes as the standard exhibits characteristic (*a*) but not characteristic (*b*), the figure being asymmetric to precisely the same degree as is the standard. Hence between S.D. (*a*) and the corresponding character of the figure recognized a positive relationship obtains, whilst the relationship between S.D. (*b*) and its corresponding character in the figure recognized must be classed as negative.



Standard figure *C* (1). C's first reproduction. C's third reproduction. Figure recognized.

Fig. 5.

The structural characteristics of the figure or figures recognized were very carefully compared with the corresponding characteristics in the successive reproductions and the outcome tabulated in terms of positive and negative relationship. These detailed results are available for inspection at the Cambridge Psychological Laboratory; examples are reproduced in Fig. 6. The material allowed also of quantitative treatment, with the following results (in the following computations, deviations in series *A* are arbitrarily assimilated to the class of Stereotyped Deviations):

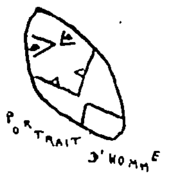
(1) *Positive relationship.*

P.D.'s in reproductions with corresponding deviations in figure recognized:

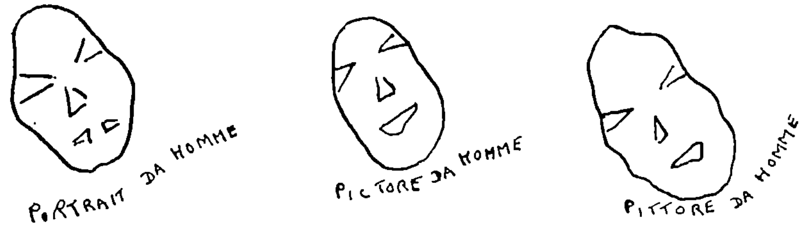
(*a*) C.D.'s = 4
(*b*) S.D.'s = 15

P.D.'s in reproductions with corresponding deviations in figures recognized (more than one):

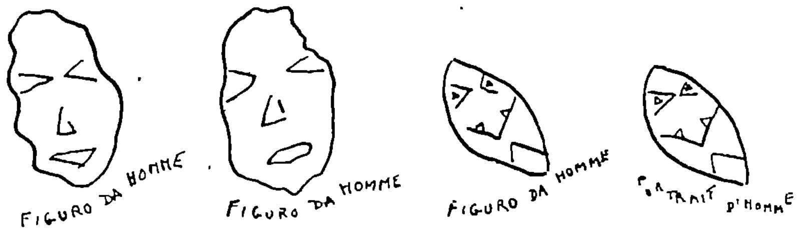
(*a*) C.D.'s = 3
(*b*) S.D.'s = 6



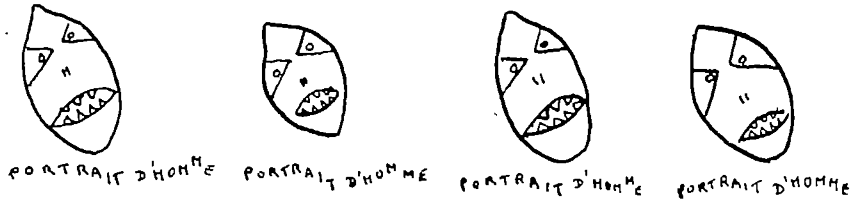
Standard figure *E* (2).



A's first reproduction. A's second reproduction. A's third reproduction.



A's fourth reproduction. A's fifth reproduction. Figures recognised:
'Yes.' 'Possibly.'



C's first reproduction. C's third reproduction. C's fifth reproduction. Figure recognized:
'Yes.'

Fig. 6. Examples of Continuous and Stereotyped Deviations and of positive and negative relationships.

P.D.'s in reproductions with corresponding deviations in *one* of the figures recognized, the other being the standard:

(*a*) O.D.'s = 6
(*b*) S.D.'s = 4

Total for P.D.'s in reproductions which show a positive relationship with corresponding deviations in the figure recognized:

(*a*) O.D.'s = 13
(*b*) S.D.'s = 25
Total = 38

(2) *Negative relationship.*

P.D.'s shown in reproductions, but figure recognized equivalent to standard as regards this deviation:

(*a*) O.D.'s = 13
(*b*) S.D.'s = 10
Total = 23

(3) *Positive and negative relationships expressed in terms of percentage of total* O.D.'*s*, S.D.'*s, and* P.D.'*s tabulated.*

Total for all cases of O.D.'s in reproductions showing positive relationship with deviations in the figure recognized:

(*a*) 50·0 per cent of total O.D.'s tabulated.
(*b*) 21·3 per cent of total P.D.'s tabulated.

Total for all cases of S.D.'s in reproductions showing positive relationship with deviations in the figure recognized:

(*a*) 71·4 per cent of total S.D.'s tabulated.
(*b*) 41·0 per cent of total P.D.'s tabulated.

Total for all cases of P.D.'s in reproduction showing a positive relationship with corresponding characters in the figure recognized: 62·3 per cent.

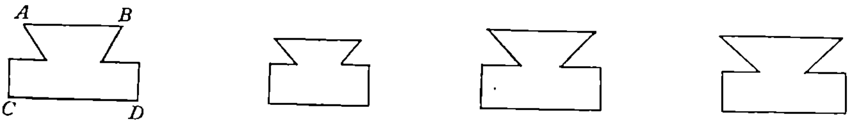
Total for all cases of P.D.'s in reproduction showing a negative relationship with corresponding characters in the figure recognized: 37·7 per cent.

(3) *The results in relation to Koffka's theory*

On Koffka's view, a trace will undergo progressive change in the direction of maximum stability. Such change is conditioned by autonomous changes, which "modify traces and trace-systems until their stresses have become as well-balanced as possible—stresses that is, within their interior and between them and their surrounding trace-systems". This concept of autonomous change incidentally transcends that originally developed by Wulf, who regarded such changes as deriving from sources inherent in the individual trace-pattern itself, as a result of its own intrinsic stresses.

Suppose a number of figures be exposed to the subject, one of which (the standard) has been previously perceived by him. Then, on Koffka's view, the present process should 'select' that trace which leads to an optimum process, and, as an inevitable concomitant, to greater stability of the trace. This, according to Koffka, represents the dynamic constitution of the process of recognizing. Now the trace corresponding to the perception of the standard has, in the interim, undergone autonomous change (in the widest sense of the term) and the pattern corresponding to the standard will not now be the pattern most similar to that of the trace. Consequently the latter may achieve greatest stability through intercourse with a slightly changed process. This would be effectively achieved through the perception of a pattern which more closely resembles the present pattern of the trace. Conversely, for the process to be optimum, this trace will be 'selected' and the new pattern becomes a 'recognized' pattern.

This line of argument assumes, of course, that the pre-eminent factor in recognition is the attainment of a condition of greater stability by the trace. If this be the case, then in every instance where Continuous Deviations occur, one would expect that particular figure to be identified as the standard which exhibits the deviations in question somewhat more pronouncedly than did the final reproduction. Such figures were, as a rule, constructed for inclusion in every recognition series, but unequivocal evidence of this prophecy being actually fulfilled was obtained in only one instance in series *B* (2) by Subject C (see Fig. 7).



Standard figure *B* (2). C's first reproduction. C's second reproduction. Figure recognized.

Fig. 7. The progressive trend towards equalizing in length *AB* and *CD* has apparently reached its final stage in the figure recognized as the original.

Moreover, the results indicate that, of all the O.D.'s manifested in the reproduction, only 50 per cent were paralleled by corresponding deviations in the figure recognized. Furthermore, it must be borne in mind that, of all Persistent Deviations observed, only 42·6 per cent were Continuous, so that but 21·3 per cent of the total Persistent Deviations are both Continuous and show a positive relationship with the figure recognized. Finally, Subject B's verdict in five cases lay between the standard and a figure exhibiting one or more deviations paralleling the P.D.'s in his reproductions. The greatest certainty in those cases by no means always appertained to the latter figure.

It may nevertheless be argued from Koffka's hypothesis that the pre-eminent factor in recognition is not that a trace should attain to a condition of greatest possible stability, but that the perceptual process should become as 'good' a process as possible.[1] Should this be the case, it might be expected that the *least* 'good' perceptual process will be the one that selects the particular trace in question.

Hence under the conditions for recognizing reported in this investigation, a condition of greater disequilibrium might obtain between the trace and the process corresponding to the perception of the *original* figure than between the trace and any of the processes corresponding to the perception of the remaining figures, in which case, of course, it would be the original figure that would be most likely to be recognized.

As we have already shown, however, positive relationships were found to obtain in the case of 62·3 per cent of all the Persistent Deviations tabulated, and this result would favour our second interpretation of Koffka's theory even less than it does the first.

We conclude therefore that:

(1) If this experiment be admitted to constitute a legitimate experimental test for Koffka's theory of recognition, the evidence obtained is insufficient to justify its validity.

(2) On the other hand, the fact that a positive relationship obtains between Persistent Deviations and the corresponding characters of the figure recognized in 62·3 per cent of all the Persistent Deviations tabulated requires explanation. Koffka's hypothesis probably indicates a better approach towards the required interpretation of this phenomenon than any as yet elaborated.

(3) The evidence submitted in this report is not claimed to be sufficiently adequate to form a basis for any but the most tentative constructive proposals.

(4) *The results in relation to Bartlett's theory*

Bartlett, as has been already observed, regards recognition as preeminently dependent upon two factors: (*a*) sensory reaction or pattern, and (*b*) orientation. Evidence seems fairly conclusive that no recognition can occur without the co-operation of both, but as Bartlett himself carefully stipulates, we cannot conclude that recognition is necessarily bound to follow when both the original sensory pattern is reinstated, and the original orientation or attitude is 'carried over' from presentation to re-presentation. These factors constitute, as it were, the essential conditions rather than the essence of the process.

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[1] I am indebted to Dr Duncker, of Berlin, for suggesting this point and for some helpful discussion of its possible implications.

Our investigation has shown that on exposure of a number of figures, amongst which is the standard, recognition as a rule follows, but that this recognition is from an objective standpoint by no means always correct.

The very fact that a process of recognition can occur at all would seem to demonstrate that the orientation condition is adequately satisfied, and we therefore seek the cause of such inaccuracy in the light of the other factor, namely, that of the present sensory pattern. Now if recognition occur when the present sensory pattern possesses but a *general* similarity with the original pattern, then the figure recognized might be any one giving rise to an approximately similar sensory pattern, and when a number of similar figures are presented for identification of the one originally exposed, identification of the standard would amount merely to guesswork. In this case there would be no reason at all, apart from chance, to expect any positive relationship between Persistent Deviations and recognized figure. On the other hand, should recognition depend on a very close similarity between original and present patterns, correct identification might be expected on every occasion.

Though systematic experimentation upon the specificity of recognition, under such standard conditions of presentation as enable the constitution of sensory patterns to be accurately controlled and varied, has yet to be carried out, there is reason to believe that for correct recognition to occur, the factor of exact reinstatement of sensory patterns may turn out not to possess paramount importance. Such a conclusion would accord well with recent criticisms of the old Constancy Hypothesis.

It may be argued, however, that Bartlett's theory does not presuppose the independent operation of each of the two general conditions, and that our exposition involves a type of illegitimate abstraction. An orientation operates selectively with regard to sensory data, and these two factors co-determine the perceptual response. Should similarity be a genuine determinant of recognition, it must almost certainly consist, not in a physiological, but in a phenomenological, similarity.

A further set of experiments is now being carried out which is specifically directed towards a study of the inter-relationship between sensory pattern and attitude in recognition. The outcome will, it is hoped, provide further material for an evaluation of Bartlett's hypothesis.

IV. CRITICISM AND SUGGESTIONS

As was initially pointed out, this investigation is essentially 'preliminary', its principal function being to indicate in the light of current theoretical formulations some possible lines of research into the psychology of recognition. The following remarks are to be taken merely as constructive proposals which might merit consideration should this research be repeated on an amplified scale.

(1) *Validity and legitimacy of the investigation.* It is essential that there should be agreement as to the significance of the results to the theories under examination. We have to establish, in fact, not only that our results are 'valid', i.e. that they genuinely represent what we believe or intend them to represent, but also that they are 'legitimate'. Thus to the present writer the experimental method employed would appear on the whole a legitimate procedure for critical examination of certain theoretical views, but permanent conclusions can be drawn if, and only if, a similar verdict is obtained from others working in the same field, and also, it is hoped, from those eminent psychologists on account of whose theories this investigation came to be attempted.

Should the procedure employed in this preliminary investigation not be accepted as legitimate in the above sense of the term, it is hoped that an analogous method be devised which would satisfy all parties. Towards this end, the following criticisms and suggestions are directed.

(2) *Criticism and suggestions with regard to material.* Extremely interesting results might be forthcoming should the experiment be repeated utilizing various kinds of material. As standards the following types of material could all be employed: Perfectly symmetrical diagrams, highly asymmetrical figures, representative drawings, colours, prose passages, arhythmic complexes of auditory stimuli, and tactual patterns.

(3) *Criticism and suggestions with regard to procedure.* (*a*) The most serious criticism that is likely to be raised against our mode of procedure is that the recognition processes may be complicated by the inclusion in each recognition series of figures closely resembling, in all cases derived from, and in some cases identical with, the subject's own reproductions. The difficulty might be circumvented by using two series of closely similar standard figures (say *A* and *B*). Successive reproductions of members of series *A* are obtained, and by analogy with the trends of change observed in these reproductions, an appropriate recognition series for each member of series *B* could be constructed. Some danger of 'Figure-assimilation' and kindred processes might, however, result, which would complicate the issue.

Nevertheless the results of the present method can be by no means wholly discredited on account of the possibility of this type of complication. After presentation of the final recognition series (*E* (2)), the subjects were instructed to answer the following question: "Are you aware that copies of your own reproductions and/or modifications of them were included in each of the ten series presented to you for identification of the originals? If so, at about what stage of the experiment did you become aware of this?" Subject A answered that he had "become aware of this" only during the presentation of the final recognition series on the last day of the experiment, but that it had occurred to him during the presentation of the *C* (1) recognition series that diagrams something like his own reproductions were included. Subject C also replied that he had "become aware of this" only in the case of the final recognition series *E* (2). Only Subject B believed that he "knew it all along".

Two distinctive factors must hence be taken into account in considering the results: (1) The mere presence in each recognition series of figures constructed on the same proportional basis, and, in some cases, on the same representational basis as individual reproductions, and (2) the explicit awareness by the subjects of the presence of such figures in each series. Perhaps the greater uncertainty and greater lack of specificity in the identifications of Subject B as compared with those of the other two subjects are to be in part attributed to the influence of the second factor.

Until further experiments have been carried out, it would seem rather fruitless to speculate as to the influence of these factors in recognizing and to the possibility of consequent invalidation of much of our theoretical criticism.

(*b*) The stereotyping effect that frequently occurs in our successive reproductions is also well shown in some recent and as yet unpublished results obtained by Grindley. It is felt that more satisfactory evidence with relation to the existence of postulated autonomous changes might be forthcoming if but a single reproduction of each standard figure were obtained. The lapse of time between exposure and reproduction would, of course, be of different duration in each case.

A comparative study of the results obtained under the following conditions could hardly fail to be illuminating:

(1) Frequent successive reproduction;
(2) Infrequent successive reproduction;
(3) Single reproduction.

In any case experiments should be prolonged over greater time intervals than were employed in the experiment reported, where the maximum interval between initial exposure and ultimate identification of a given figure amounted only to twenty days.

V. SUMMARY AND CONCLUSIONS

The object of this investigation was a small-scale preliminary study of the relationship between deviations in successive reproductions of simple figures and the structural characteristics of that drawing identified in each case by the subjects as constituting the original standard figure. It was thereby hoped to obtain some evidence as to the possible relationship between the types and trends of unwitting deviation in successive reproductions and accuracy or inaccuracy of subsequent recognition of the standard figure. More generally, the experiment may be regarded as an investigation of the inter-relationship between the processes of recognition and recall, with special reference to Koffka's Theory of Dynamic Traces and his interpretation of recognition. The main conclusions are the following:

(*a*) *As regards reproducing:*

(1) Reproducing the simplest figures constitutes a process itself by no means psychologically simple.

(2) This process typically displays an essentially constructive or reconstructive character, and with the subjects employed, reproduction was mediated pre-eminently through the agency of verbal and geometrical formulae which were memorized and recalled as a basis for reproduction of the figure. Many Stereotyped Deviations seem to be due to faulty construction on the basis of formulations which erroneously represent proportional relations.

(3) Persistent Deviations occur in every series of reproductions, and may be empirically classified as Stereotyped or Continuous Deviations. The former constitute 52 per cent, the latter, 48 per cent, of the total tabulated.

(4) Persistent Deviations may, though not without incurring the risk of artificial systematizing, be analysed in terms of Wulf's categories of 'sharpening' and 'levelling'. It is nevertheless impossible to infer in any given case that a given deviation may be unequivocably ascribed to the influence of a single causal factor. Many of the deviations could, with equal theoretical justification, be regarded as results of normalizing,

pointing, autonomous change, or of various combinations of these dynamic tendencies.

(5) Great caution must be exercised in referring changes in overt reproduction back to changes in postulated trace-systems.

(*b*) *As regards recognizing:*

(1) Where the figure recognized as the standard deviates from the latter in the same direction as a specific Persistent Deviation in the series of reproductions of a given subject, a positive relationship between Persistent Deviation and corresponding characteristic of the figure recognized is said to obtain. Where, on the other hand, a series of reproductions exhibits a Persistent Deviation, but where this deviation does not occur in the figure recognized, which, as regards this characteristic, is equivalent to, or identical with, the standard, a negative relationship is said to obtain.

Persistent Deviations may hence exhibit a positive or a negative relationship with the corresponding characteristics of the figure recognized.

(2) A positive relationship was found in 50·0 per cent of the Continuous Deviations, in 71·4 per cent of the Stereotyped Deviations, and in 62·3 per cent of the total Persistent Deviations tabulated. However, only 21·3 per cent of these Deviations are both continuous and manifest a positive relationship.

(3) In only one case did a figure recognized as the standard deviate from the latter in a more pronounced degree than did the final reproduction in a series exhibiting the deviation in question as a continuous change.

(4) If the experiment be regarded as a legitimate test of Koffka's Theory of Recognition, the evidence is not sufficient to substantiate it.

(5) With an analogous reservation, Bartlett's theory appears inadequate to account for the particular nature and direction of inaccuracies of recognition.

My sincerest gratitude is due to Prof. [illegible] for a great deal of constructive criticism and I have also to thank Dr Blackburn for his advice with regard to technique and for much helpful discussion of results and interpretation.

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THE TESTIMONY OF CONVERSATION[1]

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I. INTRODUCTORY: EARLIER EXPERIMENTS ON TESTIMONY IN GENERAL

THE progress of psychology is like that of a wave-front on a beach of varying levels. At most points the advance of the front can be easily noticed, though in a few the tide seems relatively stationary. One of these static spots appears to have been reached by the work on testimony (*Aussage*).

Any tribute to the initiating enthusiasm and subsequent energy shown in this region by Prof. William Stern (1) cannot be over-emphasized. The distinction between narrative and interrogatory, and the use of coefficients of testimony, have become familiar even to elementary students of experimental psychology. Yet, so far as we can discover from the literature, there has been little investigation concerning testimony for actual *conversation* dealing with controversial matters which provoke emotional excitement in speakers and listeners. An interesting exception is the work of Amtsrichter Dr Boden (2) of Hamburg, in 1914.

He used dialogues which were read to the listeners by actors. One dialogue was between a prospective renter of a house, the present tenant, and the wife of the landlord; another (in which the subjects were supposed to be apprentices overhearing the conversation) took place between the proprietor of a delikatessen shop and a traveller trying to overcome the proprietor's refusal to raise his prices by suggesting a dubious compromise.

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[1] This article is based upon papers read to the Cambridge Anthropological Club on 1 November 1935, and to the British Psychological Society, Northern Section, on 18 April 1936.

There seems to be no other published work upon the reliability of testimony concerning conversations. Before the experiments to be described were performed, however, Dr J. M. Blackburn and Dr E. J. Lindgren had carried out an investigation in Cambridge, employing an actual discussion of a university society. The words were transmitted by a microphone and recorded by stenographers, this being unknown to the speakers. We are grateful to these workers for kind permission to mention this investigation, at present unpublished.

Our own problems may conveniently be stated in question form. When conversation has been reported, either orally or in writing, how often have *the identical words used by the speakers* formed the material? In cases where they have not, little scientific value can be attached to such a 'record', for the reporter may occasionally have chosen words which, though not the actual ones, seemed to him to be synonymous with those spoken by the original conversers. Yet, as any educated individual whose literary sense has not been blunted by cross-word puzzles will agree, there are few real synonyms—in the English language at any rate. A study of Roget's *Thesaurus* merely deepens this impression. To pursue this line of thought would plunge us into discussion of the meaning of Meaning (3). It has been suggested (in a letter) by Prof. F. C. Bartlett, that the meaning of many a word may be more clear-cut to a visualizer, because accompanied by a visual image, than to a verbalizer, and the visualizer complains that the word which 'obviously' fits the cross-word puzzle, and is afterwards proclaimed by the Editor to be 'right', is often far from a true synonym. Possibly, therefore, our objection to many so-called 'records' of conversation may appear more serious to readers with one type of imagery than it does to others (4). Yet, clearly the question of fi[illegible]pecially important in testimony concerning words, and more [illegible]ant than when the subject-matter is visual. To take an example from to-day's news; all witnesses might testify that a speaker's shirt was of a certain conventionally labelled hue, but whether at a certain moment he said Rebels, Right-wing, Militarists or Monarchists, to indicate one side in the Spanish struggle[1] and Government troops, Popular Front, Left-wing, Socialists,

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[1] This was written on 31 July 1936.

Communists or 'Reds', to denote the other, might be important in any legal question of sedition or incitement.

Such comments, not intended in the least to be destructive, may conceivably be applicable even to the careful records of conversation made by Dr Susan S. Isaacs (5), Prof. David and Dr Rosa Katz (6) and Prof J. Piaget (7), unless each and all of these possesses reliable auditory imagery. This is not to suggest the contrary, but to emphasize the importance of discovering more about the mental peculiarities of any reporter, as Mr Gregory Bateson (8) and Dr E. J. Lindgren (9) have pointed out.

II. ARE LITERARY RECORDS OF CONVERSATION PSYCHOLOGICALLY TRUSTWORTHY?

In the above-mentioned books, the conversations recorded were seldom likely to arouse emotions due to strong sentiments or complexes. Without further evidence, one cannot say the same of all recently recorded conversations. Sir Arnold Wilson in *Walks and Talks* (10) writes: "All the characters and conversation herein recorded are real, none are imaginary.... My aim is to present a faithful, though of course, inevitably superficial impression."

Almost without exception these conversations appear in *oratio recta*, inside inverted commas. Consequently if by "a faithful impression" the author meant a faithful record, these pages would be of inestimable value to the psychologist. Perhaps the significance of this difference did not occur to Sir Arnold. Yet it is only fair to note that the first ten conversations which he has chosen to report concern matters which are debatable to many English readers and, presumably, to the author himself. These are: India (p. 1), the desirability of more births in England (p. 11), sexual promiscuity (p. 19), a police-constable's treatment of an itinerant knife-grinder (p. 30), the House of Lords (p. 36), a boxer's financial dependence upon a Jew (p. 41), less speed urged for motorists (p. 53), youth charged with sexually illegal offence (p. 66), Council houses (p. 76), "Self sacrifice...the lesson of Italy and Germany" (p. 82).

The serious question therefore arises: Can the actual words of any conversation on such exciting subjects be reported faithfully unless the words were actually set down at the time? To this presumably there is one answer: yes, if the reporter's memory for actual words, and not merely for their meanings, is not only perfect in ordinary circumstances, but is also proof against the disturbing influences of emotion, prejudice, mental censorship, selection of material with regard for presentability

(Freud's *Rücksicht auf Darstellbarkeit*), leanings towards dramatic elaboration and artistic or literary reluctance to present an incomplete story; a matter of special interest to the Gestalt psychologists (11).

We emphasize that in the present paper the forensic aspect of the subject, serious as it may be, will be held apart from and regarded as subordinate to the purely psychological aspect. We are concerned here with the degree of correspondence of the testimony with the facts, and the reasons for divergences.

III. TESTIMONY EXPERIMENTS USING VERBAL METHODS

The next question is: How far has previous *experiment* contributed to our knowledge of this particular section of the subject of testimony? Dictated matter has been used, as by G. M. Whipple (12), Ll. Wynn Jones (13) and Luetgebrune (14). The good points of these investigations are, however, partly offset by one defect; their matter lacks vividness and vivacity, and not only this, but also the unexpectedness of the give-and-take between two minds. If, for instance, a stage-conversation in Shaw's *Pygmalion*, Schnitzler's *Anatol*, or Noel Coward's *Private Lives*, be compared with a passage read unemotionally in the laboratory, it is clear that these constitute entirely different social situations, since in the laboratory-reading, the factors of emotion, rapid changes of attention and excitement are minimized or even absent. One should, however, remember that stage-conversations seldom resemble the real thing.[1] On the other hand, since people seek real conversations so avidly one can scarcely regard them as uniformly dull.

It seems doubtful if much is known about the factors involved either in recalling or in listening to conversation. Would the difficulty in giving correct testimony of such events increase with the number of conversationalists? Is a conversation in which one has taken part easier or harder to report faithfully? Casual consultation about this matter with different friends suggests that differences in mental make-up may play an important part here.

Two articles in the *Archiv für die gesammte Psychologie* bear upon our subject: Rechtsanwalt Dr Luetgebrune's "Parteifunktionär und Zeuge" (14) and Prof. Karl Marbe's (15) comment upon it. The first paper is an interesting commentary, by a legal writer with psychological interests, upon an anti-war speech made at a German war-memorial. The high party-feeling of the spectators (two processions, holding different views towards war, had converged) made it unlikely that anyone could have reported correctly what was actually said. There was in fact, to quote the characteristically detached summary of *Psychological Abstracts*, "a misunderstanding of phrasing due to emotional tension, accompanied by corresponding suggestibility towards certain ideas".

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[1] A prolonged study of the 'real thing' seems obviously desirable.

Marbe's article briefly reports an experiment carried out upon nineteen subjects by Maria Schorn. She read a political speech "over a house-telephone functioning very well". The subjects were told that it was part of a festival speech from the Socialist journal *Vorwärts*. They were shown a copy of this journal with the remark, "You know that *Vorwärts* is a Socialist paper", and were instructed to note carefully what was read on the telephone. After this they had to write down what they had heard—a spontaneous report or 'narrative'.

The whole text might conceivably have been taken from *Vorwärts*, except for the sentence, "Die Republik hat solche Schönrednerei grossgezogen", which directly contradicted the sense of the rest of the speech. Instead of "Die Republik" two subjects wrote (1) "Die Monarchie"; three, instead of "Wir lassen die Monarchie" wrote (2) "Wir hassen die Monarchie".

These two errors were then made the basis of 'catch questions', which were given to subjects who had not made these mistakes. They were asked: "Did the phrases (1) and (2) quoted above stand at the beginning, the middle or the end of the text?" Twelve subjects gave (1) and ten subjects gave (2) a definite position in the speech. Of the subjects who said that they did not remember (1) or did not know which part of the text it occupied, none noted that its sense was contradictory to the rest of the speech. Concerning (2), two subjects said the sentence was never read, and four that they could not remember it.

A number of sentences were noted which harmonized or were compatible (*verträglich*) with the sense of social democracy but were quite foreign to the text as read.

IV. HOW MANY PSYCHO-THERAPEUTIC CONVERSATIONS HAVE BEEN SATISFACTORILY RECORDED?

The above accounts suggest questions which appear to have been discussed very rarely; e.g. do any verbatim records exist of free association which lasted many hours? If so, how were they taken down? If by a stenographer visible to the speaker, could the words recorded have been the result of really *free* association? If words are merely written down, however faithfully, they can give their reader little idea of the important characteristics of spoken words when heard by anyone familiar with the language—characteristics such as the relative duration of the words and phrases, the intonation and speech-melody (16,17). One's judgement of these, when hearing speech, often leads to the impression that the speaker is showing doubt, indecision, sincerity or the reverse, eagerness or vivacity. No unmarked manuscript can render these characteristics. In this connexion, it is interesting to read Mr Harold Nicolson's remarks about sincerity at the microphone ((18), p. 174 f.) and the comments which they evoked (19) ((20), pp. 63–77).

If dictaphone or gramophone records of psychoanalysis exist, and if, while making them, the speaker was aware of the necessity of speaking clearly and articulately, was he really associating freely?[1] If there are any blattnerphone records (records made electrically on steel tape) of psychoanalysis they are unlikely to be long or numerous because of their great expense. If the analyst writes down the free associations from memory after the patient has gone, have they not then suffered the double censorship of his own mind and the patient's?

So far as we are aware, our procedure resembles that of predecessors in that Stern's and Whipple's methods of treating the data were employed, but differs from it in that the material for testimony was a conversation, specially written, spoken for and recorded on the gramophone.

V. REASONS FOR MAKING AND USING A SPECIAL GRAMOPHONE RECORD

It may be useful to summarize some advantages and disadvantages of using, for our special purpose, a gramophone record. Supposing that the record were perfect (and the one finally chosen was far from that):

(1) It presents, not soliloquy, but conversation, and this is not always easy to arrange without notice.

(2) A subject which interests most ordinary people can be chosen.

(3) It can involve a real clash of opinions, and tend to draw most

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[1] At the time when one of us made the dictaphone records of free association in the analysis of dreams (21) he had been using the dictaphone habitually for long periods and with considerable ease, so that it may be said that he almost forgot its presence when speaking, though it is probable that one always speaks with special attention to articulation when using the dictaphone. This ease, however, is rare when the dictaphone is used casually, without long practice. When making gramophone records, on account of the relatively expensive material (until recently) and the annoyance of making a bad record, there is probably a widespread tendency to be extremely careful in speaking. With improved methods of cheap and convenient reproduction these difficulties should rapidly vanish.

listeners into its mental arena. Affective colouring can be deliberately provoked, even if it is not necessarily similar for all listeners.

(4) It can be reproduced in approximately the same conditions for numbers of people at different times and in different places.

(5) If desirable, it could be broadcast, thus making possible the use of very large groups.

(6) Its success can be judged by many people, expert or otherwise, at different times. Its script could be written and recorded by experts in both branches of this new art.

Disadvantages are:

(1) The speakers are not seen. Yet conversation on the radio, very common nowadays, is of this type, and presumably overheard conversations between unseen people (e.g. 'tapped' conversations on the telephone) may be very important from the legal standpoint.

(2) Some people, not really defective in hearing, find a difficulty in listening to 'mechanical' speech. This will be lessened by improving the speakers' technique, and the methods of recording and reproducing. It should be added, however, that some older hearers seem to display towards gramophone reproduction what one of us has described elsewhere (20) as affective stupidity.

(3) A real conversation is usually 'untidy'; the speakers overlap, interrupt, rapidly change the tempo of their speech. Some judges of the record used in this experiment have said that it sounded too rehearsed and 'tidy', too regularly spoken and easy to follow. Others have disagreed, pointing out that plenty of real conversations are relatively 'tidy', and regular in form. Actually, there are no interruptions in the record used, though they could easily be made.

(4) The rehearsed conversation is often too logical in its sustained argument to be real. (It is, of course, not difficult to write an illogical conversation.)

The general aim in making the record was to produce a conversation which (*a*) can be understood by most people, (*b*) while not exceptionally exciting, concerns a subject of undoubted popular interest, (*c*) is unobtrusively filled with ideas and arguments which lend themselves to numerical recording, (*d*) contains some expressions of opinion with which any one listener is likely to sympathize, and others which will probably arouse antipathy in him,[1] (*e*) uses, on the whole, ordinary methods of expression, but here and there a few which are unusually phrased, and might give rise to interesting misquotations, (*f*) might contain one or two 'catches', i.e. phrases or words which are out of key with the rest of the conversation, and, when remembered, might be distorted in order to make sense.

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[1] We trust that American readers will not be unduly pained by the views expressed by one of the speakers, but this topic succeeded in arousing interest and was, of course, included for that reason only.

VI. THE CONVERSATION USED

Speakers: *A*, a woman, *B*, a man.

(*Waltz fades down.*)

B. Well, that's over; what about a cup of tea?

A. Good idea. Didn't you enjoy it?

B. Not terribly.

A. Don't you like the pictures?

B. I used to, when they didn't talk, and had real music. Nowadays, after half an hour, I get bored—I'm sorry.

A. Why? You can't be normal.

B (*interrupts*). Oh, I say!

A. Ordinary, I mean, I beg your pardon; look at the hundreds of people queuing up outside the cinemas—

B. Yes, look at them—have you ever?

A. Don't be silly, I'm there sometimes.

B. Ah well, that does alter things a bit. But don't you think those who queue up just take what's given them on the screen, along with the really important things?

A. What are they?

B. Warmth, comfortable seats, darkness, and a complete get-away from the day's washing, the account-books, the dull streets, the crowded home—

A. Don't you ever try to escape from *your* work?

B. Yes, of course, and the pictures offer a short way. But after a few minutes I often find them too dull to distract me. Don't you?

A. Sometimes—but I never go to the pictures unless I want to see some particular film. I hate the American voice, and so they are nearly all British films that I see.

B. Aren't you being a bit narrow-minded?

A. No I'm not; to me the average American film-actor's voice sounds hideous.

B. So really there's something to be said for the old silent films after all.

A. Well, at least they couldn't utter some of the noises we hear nowadays—on the other hand they couldn't give you Elisabeth Bergner, Seymour Hicks, or Charlie Chaplin[1] or—

B. —the all-talking, all-singing, all-dancing atrocities.

A. I agree with you there, but you needn't go to see them. What about Mickey Mouse and the News Reels—and some of the 'shorts', so grimly labelled 'educational'; surely you can't be bored with those?

B. No, I like them. What about the music? Do you like canned music and the cinema-organ?

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[1] The last name was inserted to discover how many listeners would note this contradiction.

A. Well, it may be canned, but it's better than the old tinkling pianos or the local orchestra with 'effects', which supported the silent films. As for musical settings; did you see *Sanders of the River* or *Bulldog Jack*?

B. No, I didn't. Some of the films I've seen lately have been very cleverly made, but that doesn't alter the fact that they still bore *me*.

A. Well—all I can say is that either you go to the pictures too often to appreciate what you see, or too little to find out what you're missing. You must admit that the cinema gives you thrills.

B. But what kind of thrills? Do you really get excited now at the pictures when you see and hear a revolver shot off, or a car hit a lamp-post?

A. Yes, I do, if the thing is well done, that is. I know there are a lot of second-rate dramas in celluloid, but these are the films I don't queue up for.

B. Even then, you make the queue only one less. Why do so many thousands go to the pictures regardless of the film—I still hold that it is the warmth, and all those things I mentioned, which attract them.

A. Maybe you're right—I must say I like to enjoy the film from a comfortable warm seat when I've taken so much care to choose it. Those who aren't so particular would probably get infinitely more enjoyment from their visit to the pictures than you or I.

B. Well, I hope they do, I'm afraid I shall never be a film fan. Look at 'fan'! What a mess they're making of the language too!

A. Well, we can't discuss that now. Here's our 'bus.

VII. DESCRIPTION OF EXPERIMENT

The subjects of the investigation

Three different types of subject were employed: (A) university undergraduates, (B) members of classes of the Workers' Educational Association, (C) students at St Peter's Training College for Teachers, Peterborough.[1]

The subjects sat near the portable acoustic gramophone. The university students sat in their usual places in the lecture room and appeared to hear well. In the two Workers' Educational Association classes, the experimenter asked those who had not normally sharp hearing to come nearer. One or two of them, usually older people, reported after the experiment that they did not hear well. Their scripts were eliminated from the number used for calculating the results. Paper and pencils were given out, and the instructions (read by the experimenter from a typewritten sheet), were:

"We are interested to know *what* you can remember of a conversation, immediately after hearing it. Obviously, the best conversation would be a real one, but by using a gramophone record we have the chance of comparing your answer with others. It is not a very good record, and would you very kindly listen as if you were overhearing a conversation on the radio, or in circumstances where hearing is a little difficult? If the record stops suddenly, try not to laugh, and at any moment when I say 'Now!' please write down all you can remember of the conversation." (This was inserted so that if the record broke down, the conversation up to that point could be reported.)

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[1] We wish to thank Miss Valentine Hall very warmly for kindly carrying out the investigation at Peterborough.

"It is supposed to be between two people coming out of a cinema, and begins with some rather bad cinema music. Don't listen critically, either to the quality of the sounds made or to the conversation. There are two speakers, a man and a woman.

"Immediately I say 'Now!' write down everything you can of the conversation you have heard. There is no time-limit. After you've written down all you have heard, please look at the blackboard for further instructions."

The instructions were written on the blackboard while the subjects were writing their reports. The blackboard then displayed the words:

Underline every phrase of which you are absolutely certain; e.g.

$\left.\begin{array}{l}\text{The dog was}\\ \text{in the street}\end{array}\right\}$ is two phrases.

VIII. SCORING OF RESULTS

The typescript from which the record had been made was divided up into its component items. This was done by separating the text into 'ideas', or what appeared to be distinct thought-entities, whether conveyed in a whole sentence or in one word. For instance, the sentence "Aren't you being a bit narrow-minded?" was taken as one item, since one idea was conveyed. "Warmth, comfortable seats, darkness, and a complete get-away from the day's washing" was counted as 5.

The separation of items was carried out on the same plan. The judgements were quite arbitrary, but the system lent itself to comparable results which could be fairly checked and easily counted. Word-for-word analysis was contemplated, but in a record of this length was found to be well-nigh impossible.

There was a total of 132 items. Each report was divided similarly. The totals were counted, also the total number of items which were underlined as subjectively certain. Each was compared with the original, and the items reported were put into three categories, *right*, *synonymous* (or making fair sense) and *wrong*. Of the number underlined as certain, there were two categories, right and wrong. The margin of error allowed in each item recorded as correct was confined to prepositions or conjunctions which did not essentially alter the sense; e.g. "Well, that's over now", instead of "Well, that's over". The correct items were marked with a tick (check) and those definitely incorrect with a cross.

The unmarked items were counted as those which conveyed the right gist or trend, synonymous with or making fair sense out of the original material. Correct items, grossly out of place in the logical sequence of the argument, were counted wrong, as were those attributed to the wrong speaker. The number of crosses and ticks gave the number of right and wrong items, and the total, minus these, gave the amount of gist.

The figures obtained for each script were tabulated, and from the results each individual's reliability and accuracy of report, and subjective assurance, were calculated. The methods for finding these were taken from Whipple (12):

P = no. of possible items,
n = no. of items reported,
c = no. of items reported *certain*,
$n(r)$ = no. of items reported correctly,
$n(s)$ = no. of items making fair sense or meaning,
$n(w)$ = no. of items reported wrong,
$c(r)$ = no. of items *certain* and right,
$c(w)$ = no. of items *certain* and wrong,
$\frac{n(r)}{n}$ = accuracy of report,
$\frac{c}{n} \times 100$ *Percentage Subjective Assurance*,
$\frac{c(r)}{n} \times 100$ *Percentage Correct Subjective Assurance*,
$\frac{c(r)}{c} \times 100$ *Reliability of Report.*

The number of scripts used from Group A was 32; from Group B, 15; from Group C, 44. The actual number of subjects was greater than this, but some scripts were reported in 'indirect oration'; i.e. their writers had not obeyed the instructions. Other subjects did not hear satisfactorily.

Table I shows comprehensively the relative range and accuracy of the three groups.

Table I. *Relative range and accuracy of the three groups*

Group	Percentage range n	Percentage correct sense $n(r)+n(s)$	Percentage verbal accuracy $n(r)$
A	7·5–61·3	4·5 –30·3	0·75–14·3
B	9·1–33·3	5·03–24·2	3·8 –13·6
C	15·2–44·6	6·9 –33·3	1·5 –13·6

Though we know of no figures with which to compare them, these results seem to us to be low, particularly where the general sense of the conversation is reported with so little accuracy. Many people have not caught the thread of the argument, such as it is, at all, and many have reversed entirely the opinions given in the record. Others have recollected phrases here and there, stringing them on a thread of their own manufacture. The university students often use—sometimes inappropriately—sophisticated 'synonyms' for the ordinary words deliberately chosen and actually spoken.

The items which were reported wrongly, i.e. the interpolations, were interesting: e.g. cups of "coffee" instead of "tea", "people with big mouths making crude noises", "silly high-brow orchestras" instead of "local orchestras" may represent the elaboration in increasing degree.

IX. CONCLUSIONS

The range seemed to us surprisingly small, the maximum being 81 and the minimum 10 items, out of a possible 132. One may perhaps surmise that the material was too 'abstract' and difficult for the subjects to remember. We have no particular evidence for this assumption, which may be incorrect. Yet a possible beginning for future work might be to investigate whether different types of conversation (concrete, abstract, etc.) are remembered and reported more easily by different types of person, taking into special account differences of imagery and of environmental influence. Is there anything, for example, in the belief that simpler minds tend to remember actual conversations, and that, as education proceeds, 'meanings' are preferentially recalled, or does the less sophisticated person tend to dramatize, and thus to report in conversation form, the truths which the philosophically trained would easily state in terse, familiar verbal formulae?

Table II shows the subjective assurance of the subjects.

Table II. *Relative assurance and reliability of the three groups*

Group	Mean value $\frac{c}{n} \times 100$ Subjective assurance	Mean value $\frac{c\,(r)}{n} \times 100$ Correct subjective assurance	Mean value $\frac{c\,(r)}{c} \times 100$ Reliability
A	26·06	13·3	45·8
B	52·04	30·4	53·2
C	40·4	16·85	39·3

Six subjects out of ninety-one underlined nothing at all. A possible reason is that some subjects misunderstood the instructions. We think this improbable, since four of the six were university students, accustomed to carry out instructions given in class, and the experimenter remembers giving them with the utmost possible care.

Group B was the most assured of the accuracy of its recall, and the most reliable in its subjective assurance. Group C was the next most assured but the least reliable. Group A was least assured but was fairly reliable. This last result may be accounted for by the fact that the group was trained psychologically to a certain extent, and more liable to regard the experiment objectively and impersonally. Knowledge of their mental limitations by the members of this group may have tended to reduce their subjective assurance. The greater average age of Group B may have affected their reliability.

Objective assurance is not necessarily proportional to subjective certainty. This is shown by the low correlations: 0·325 with P.E. 0·084 for Group A, and 0·484 with P.E. 0·07 for Group C.

The results show no consistency, either between the groups or within them. The great differences between the groups caused by training, background, attitudes and more personal endowments show up more in the qualitative than in the quantitative aspect of recall. Group A, on the whole, recalled the skeleton of the argument, which was more or less correctly elaborated. Group B gave less elaboration and a few of the points, but on the whole was more accurate. Group C presented disjointed argument with a great deal of elaboration. Out of the ninety-one subjects, forty reported more incorrect than correct material.

The reasons for the gross amount of inaccuracy must be many and various and are probably based upon individual differences in memory and imagery, as well as upon environmental factors of education and upbringing. The necessity of first recalling and then transferring the conversation to paper must have introduced several variable factors: e.g. some people like and others dislike expressing themselves in writing. The value of carrying out similar experiments on a questionnaire basis, with and without free recall, is under consideration. Whether the variability is due to the experimental procedure or whether such a measure of error is prevalent in the recall of any everyday conversation can be settled only by further experiment, there being no figures known to us for comparison.

The three experiments recorded here were performed in different circumstances, but in each instance precisely the same directions were given. So far, the results are too variable and have been obtained from too few subjects to permit conclusive interpretations, but already they suggest a variety of problems, in a wide field, which promise profitable data for theory and practice. Some of these problems are:

(1) To what extent does success in reporting conversation faithfully depend upon the reporter's predominant type of imagery?

(2) The subject of remembered conversation would seem to lend itself to investigation under the four headings of Impression, Retention, Recall and Recognition. The influence upon all these processes of the conversation's emotional colouring should be investigated, particularly the effect of the listener's strong agreement or disagreement with the opinions expressed.

(3) The effect upon testimony of the reporter's having taken a prominent part in the conversation, or having merely formed one of an audience, should be closely investigated.

(4) The method of recording the testimony, e.g. speaking to a person who is not making notes, dictating to a stenographer or stenotypist, dictaphoning, speaking naturally near a steel-tape or similar recording apparatus, or speaking with care when making a gramophone record, writing, or typing, must alter the testimony; to say nothing of the effect of the interrogatory. A wide field of investigation exists here.

(5) In view of its forensic significance, the effect upon testimony of requiring evidence concerning conversation to be given on oath, should be examined in detail, with special reference to the mental make-up of the witness.

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SOME EFFECTS OF STYLE IN READING

By R. W. PICKFORD

(*From the Psychology Department, Glasgow University*)

I. *Introduction* (p. 292).
II. *Apparent discord of style and idea* (pp. 292–293).
III. *Apparent harmony of style and idea* (pp. 294–295).
IV. *Summary* (pp. 295–296).
Passages read (p. 296).

I. Introduction

Various mental processes in reading were recently discussed by the writer upon the basis of reports by, and observations upon, twenty-eight subjects who read specially chosen short passages of prose and poetry under standardized conditions.[1] There were thirty-one passages altogether. The subjects were given no instructions which would prejudice them or set up tendencies to report upon certain aspects of the matter read or of their own mental processes, but, quite spontaneously, out of the many reports obtained, a number were directed towards the problem of style. On account of the spontaneity of these reports, it is worth while to discuss four of them in this short paper, two given by one subject, and one each by two others. They deal with three passages of reading matter, to which references are given at the end of the paper. Here the three subjects will be numbered 1, 2 and 3, and it is to be noted that Subject 1 was interested in mediaeval literature, Subject 2 in psychology and Subject 3 in ancient philosophy. The pieces read will be called the (Henry) James passage, the Whitehead passage and the Frazer passage, respectively.

II. Apparent discord of style and idea

Sometimes when a reader finds difficulty in apprehending a passage of prose or poetry the difficulty may seem to centre about the matter intended to be expressed. At other times the difficulty is mostly caused by the style, or technique of expression, and the reader believes that he would be able to understand the matter if better written.

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[1] *Brit. J. Psychol.* (1933), xxiv, 50; (1935), xxv, 417; (1935), xxvi, 49.

Subject 1 had great difficulty in understanding the James passage. "First impression was one of extreme irritation at the barbarous style; very hard to concentrate on it with the barrel-organ outside." The barrel-organ was an unfortunate disturbing influence which the experimenter would have preferred to avoid, but no doubt the subject's difficulty with the 'barbarous style' made him abnormally sensitive to the noise. He turns his attention to the style, and criticizes it, pointing out how his difficulty lies in its lack of structure (for him). "The style is turgid and the English split up; it isn't held in—like a blousy woman. There is no architectonic structure; you feel the man's got a fuzzy mind; added to which it could have all been said in a few sentences, instead of all that windy rhetoric." He discusses the matter expressed, however, in an entirely different way, pointing out that it is comparatively straightforward. "I imagine I got the central idea all right. I can't imagine myself reading any more by the same author. It made sense to a certain extent; I could see what the man was getting at, but the thought was strained out—I read the last bit two or three times I think." On the whole, therefore, his reaction to the passage is to make some sense out of what he thinks the author intends to say, but to be unable to assimilate it in the way it is expressed. In so far as he grasps the meaning of the passage at all, he does so in spite of the style, and not through its assistance. The style is incompetent to express the idea clearly, though the idea does not present great difficulty in itself.

Subject 2 gave an interesting report on his difficulties with the Whitehead passage. In general it is the reverse of the report just considered, as will be seen. Two readings were allowed. "When I first read it I was saying, 'Oh! Lord! I shall never be able to remember this'. I was all the time completely in the dark as to what he means by 'object' or 'event' or 'ingression into'." These comments indicate immediately that the ideas expressed in the piece were incomprehensible to the reader. His next remarks make his response more explicit. "In general I felt slight antagonism, which increased—I don't like it because it pretends to be extraordinarily accurate in language-form without giving me any accurate ideas at all." His final remark sums up the whole of his response in the form of criticism of the passage given from his point of view. "It is an example of apparent clarity of style which gives a false impression of clarity of thought." For him, in fact, the style is clear but the matter nevertheless unintelligible.

III. APPARENT HARMONY OF STYLE AND IDEA

Under other circumstances the discordant relations, for given subjects, of the style and the matter expressed, seem to be naturally resolved, or not to arise at all, and then it is possible to observe certain aspects of the synthetic process of apprehension, into which style and matter enter, together with influences from the reader's own mind.

When Subject 3 was reading the James passage, he seemed "to stumble over the rhythm". The report shows a transitory phase in apprehension. "I found it fearfully difficult to take in. I can tell roughly that what I'm not able to take in is not very far off the mark." At the start the subject distinguishes between the ideas that he supposes to be contained in a given sentence, and the preconceptions or working hypothesis with which he approaches the piece. "At points the impression that I got from a given sentence—the ideas I supposed to be contained in a given sentence—seemed to correspond with, or to be identical with, the ideas that I had accepted as a working hypothesis." Then he turns to the question whether the words gave the impression the author intended them to convey. "The rest I didn't take in at all; the impressions formed round certain words, but I don't know whether they corresponded with what the author meant at all." He concludes by pointing out that the reading excited his curiosity and left him with an unsatisfied desire to understand the passage. "I want to read each sentence three or four times. I feel enormous curiosity about that; I should like to worry its meaning out for a long time, to see whether it would throw any light on my preconceptions of the moment."

In order that the passage shall be fully understood, it is necessary that the subject's preconceptions or working hypothesis, the form of expression and the ideas expressed, shall harmonize. The threefold synthesis is, however, only partly completed. The union of the reader's impressions with his preconceptions is moderately complete, but the relation between the impressions gained from a given sentence and the ideas which it is supposed to convey remains vague. The impressions were formed about certain words, and it is not clear that they were precisely the impressions the author intended to convey. In other words, the style, or the form of expression, when seen from the reader's point of view, is intermediate between his preconceptions and the matter to be expressed, and, as in this example, sometimes may not perform this function of translation successfully at first. The reader remains, therefore, in a state of curiosity, with the desire to complete the synthesis. It is interesting to notice that he seemed to find difficulty with the rhythm of the passage, which suggests that the synthesis may really be fourfold, involving rhythm as well as the other factors mentioned.

The same subject, when reading the Frazer passage, was "amused; falls into the swing of the phrases". The processes of synthesis take a different form here. "I'm tempted to accept his explanation without going deeply into it! I think it needs a mathematical mind to follow each step at all quickly." Evidently the reader is not tending to adopt a critical attitude. In some way the passage has silenced his criticisms, and this is explained in his next sentences. "It's a good example of prose that has been entirely shaped by the form of the matter, I think. But I can't follow the steps of his reasoning; I should have to read it several times through." His criticism is silenced by the clear harmony of style and idea, and he falls back automatically on the excuse that he has not a sufficiently mathematical mind to follow the argument. "I could not reproduce the structure of the argument in the very slightest; I merely got an impression that what he was saying was logically sound, because what he was saying seemed to have a mathematical rigidity. One gets the impression that it couldn't have been put either less clearly or more clearly." Finally, he adds, "I have a personal lack of interest in it that led to the slipshod reading", and this comment suggests to the psychologist that the reader had, in this example, no preconceptions regarding the meaning of the passage—another possible reason for lack of criticism, favouring the apparent clarity of the style. The psychological conditions of this reading, including the reader's lack of interest, led to his impression of a very complete synthesis of style and idea, and this was at the expense of the third part of the synthesis—between the mode of expression and his preconceptions—which would have been necessary to full understanding.

IV. SUMMARY

A number of reports made by subjects who had read short passages of prose and poetry dealt spontaneously with style. On the basis of these reports, it seems that, for a given subject, the style of a piece may fail to express ideas which are nevertheless clear in themselves, or, ideas which are inherently obscure may be expressed in clear style. Even when there is harmony between style and ideas, however, it appears that a further harmonizing of the impressions made by the style and the reader's working hypothesis of the meaning of the passage is necessary for full understanding. Peculiar completeness of synthesis of style with ideas to be expressed may silence criticism, and give the reader the impression that the passage is exceptionally clear though he does not understand it himself. It appears that style functions as a symbolic system mediating between the reader's and writer's ideas, and if this is true it would have to be distinguished from rhythm, which seems to be a directive process or activity.[1]

In conclusion, I wish to thank Prof. F. C. Bartlett, Dr R. H. Thouless, The Medical Research Council and Emmanuel College for their help.

PASSAGES READ

HENRY JAMES, *The Portrait of a Lady*, Macmillan, 1921, pp. x–xi, from "One had had from an early time" to "poor and ungenerous one".

A. N. WHITEHEAD, *The Concept of Nature*, p. 66, from "The ingression of an object into an event" to "the ingression of objects into events".

J. G. FRAZER, *Man, God and Immortality*, Macmillan, 1927, pp. 176–7, from "But in whichever way the exchange of women" to "consanguineous marriage".

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[1] *Brit. J. Psychol.* (1935), xxv, 421–3 and 424–5.

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THE VARIATION OF SENSORY THRESHOLDS WITH THE RATE OF APPLICATION OF THE STIMULUS

III. THE DIFFERENTIAL THRESHOLD FOR INTENSITY OF LIGHT

By G. C. DREW

(*From the Psychological Laboratory, Cambridge*)

I. *Introduction* (p. 297).
II. *Description of apparatus* (pp. 297–299).
III. *Calibration of apparatus* (p. 299).
IV. *Experimental procedure* (pp. 299–300).
V. *Results* (pp. 300–301).
VI. *Discussion* (pp. 301–302).
VII. *Summary* (p. 302).
References (p. 302).

I. Introduction

This experiment forms one of a series in progress at the Cambridge Psychological Laboratory, on the variation of the threshold for various sensory modes in relation to the rate at which the stimulus is changed. Accounts of two similar experiments, on pressure (Grindley, 1936 *a*) and on touch and pain (Grindley, 1936 *b*), have already been published. In the first of these, Grindley has stated the reasons which led us to undertake the experiments and has discussed their relevance to threshold determinations in general.

In the present experiment the subject looked at an illuminated field subtending an angle of approximately 5°, the differential threshold for increased brightness being measured from an initial illumination of 1·5 f.c. The rates of change of illumination were such that a just perceptible increment occupied from 0·01 to 5·0 sec.

II. Description of apparatus

In the design of this apparatus two separate light sources were used to illuminate a common field, one of them providing the standard illumination and the other a variable percentage of the standard. A shutter moving across the variable source increased or decreased the illumination of the common field at a controllable speed.

Fig. 1 is a diagrammatic representation of the apparatus. Two windows, W_1, W_2, separated by a narrow strip, were cut in a wooden screen, separate light sources, *L*, *L*, behind each window eliminating any shadow from the strip. The amount of light passing through W_1 was controlled by a metal shutter *S* moving vertically upwards over it. The shutter was rigidly secured to a microscope adjustment. W_1 and W_2 were made of such relative dimensions that a 1 mm. movement of *S* corresponded to a 1 per cent change in the total illumination. A lens fixed to *S* and a pointer moving over a millimetre scale made possible accurate adjustment to within 0·5 mm.

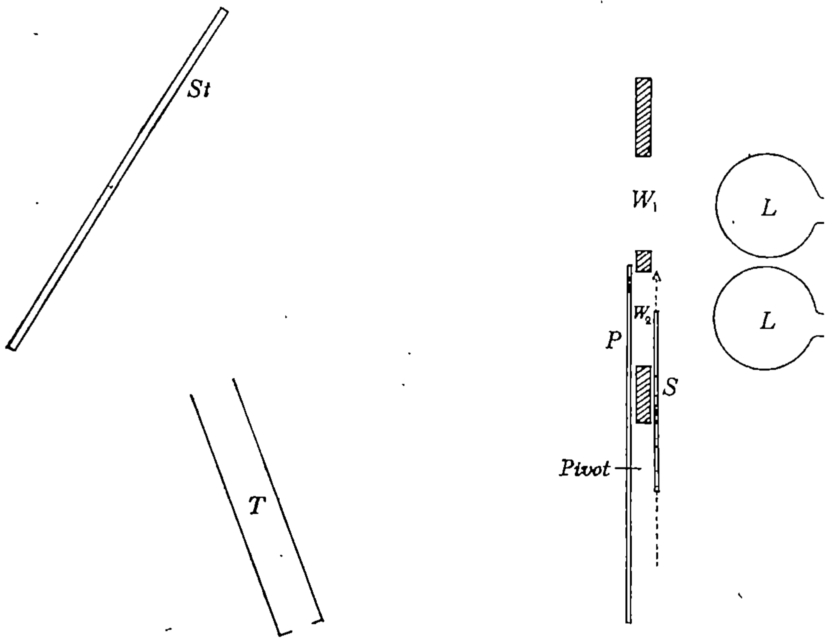

Fig. 1.

Varying speeds of change were obtained by means of a compound pendulum (*P*) moving over W_2 at right angles to the movement of *S*. The shape and dimensions of this pendulum allowed it to be activated by gravity, a light spring or an electric motor. The spring was used for very fast speeds, the shutter being projected like a catapult across W_1, and the motor for very slow speeds. Intermediate speeds were obtained by the adjustment of weights on the pendulum. Rubber stops made the movement of the shutter practically noiseless, except at the highest speed when the catapult action made an appreciable sound, while suitable damping arrangements at the end of the swing prevented rebounding.

The subject was asked to look through a tube *T* about 6 in. long, with an aperture of ¼ inch diameter, at a plain white reflecting sheet *St* placed at an angle to the windows. The tube was introduced to eliminate peripheral stimulation, but this restriction of the field of vision was found to increase the threshold considerably for any given rate of change. The variation of the threshold with the size of the area stimulated was not investigated.

III. CALIBRATION OF APPARATUS

Speed of shutter. The calibration of the speed of the shutter movement presented very little difficulty. Seven speeds, ranging from 0·01 sec. to 5·0 sec., were arbitrarily chosen as giving representative points on the graph. Of these, the higher speeds, from 0·01 sec. to 0·1 sec., obtained by a spring or by gravity, were measured photographically, while the slower, from 0·5 sec. to 5·0 sec., were calculated from the speed of the motor and the value of the reducing gears used. These calculations were verified by timing the shutter over its whole movement with a stop-watch, and calculating from that the time taken to pass the window. The speeds chosen were such that the times occupied by the change were 0·01, 0·04, 0·1, 0·5, 1·0, 2·0 and 5·0 sec.

Percentage value of change. The percentage of the standard illumination passing through W_2 was measured from the position of the subject's eye with a Holophane Lumeter. W_2 was first closed completely and the illumination of the sheet by W was measured. W_1 was then closed and the illumination of St by stray light was ascertained. This, though quite small, was sufficient to make accurate measurements of 1 per cent illumination from W_2 difficult. Measurements were therefore taken at 10 per cent, 20 per cent, etc. As these values were found to coincide with the calculated appropriate movement of S, viz. 10 mm., 20 mm., etc., it was considered that a 1 per cent illumination would be accurately obtained by a 1 mm. movement of S. The calibration of the illumination of the sheet was checked frequently during the course of the experiment.

IV. EXPERIMENTAL PROCEDURE

Subjects were asked to arrange themselves as comfortably as possible and to use the same eye throughout the experiment. All readings were taken at one sitting. The subjects were told that the illumination of the sheet would change, sometimes getting brighter and sometimes dimmer. They were warned that there would not necessarily be a regular alternation of brighter and dimmer and were asked to report any change in whatever direction it occurred. Actually, for the purposes of the experiment, records for the perception of increased brightness only were taken. The Limiting Method was used to determine the threshold. Immediately before each change the experimenter said 'Ready' and, at the two slowest speeds, said 'Over' as soon as the change was completed. Occasionally a delay was introduced between the 'Ready' and the shutter movement, or the shutter was moved without any warning signal, to see if the subject was reacting to the signal rather than to any observed change in the field. The subject was given a short rest between the determinations of the threshold for different speeds. The threshold for the fastest speed was determined first, working through to the slowest.

After the series had been completed, one of the fast speeds was again tested to see whether any marked shift had occurred during the experiment. No significant change was found.

V. RESULTS

Ten series of threshold determinations were made. Fig. 2, in which the threshold is plotted against the time taken for a just perceptible increment, shows the average curve of the ten subjects, and also typical individual curves. In the majority of cases the threshold for the higher speeds was very sharply marked, though subjects sometimes reported that they could see a 'flicker' before they could say in which direction the change was occurring. With the two slowest speeds, however, and particularly with the 5 sec. change, the threshold became somewhat

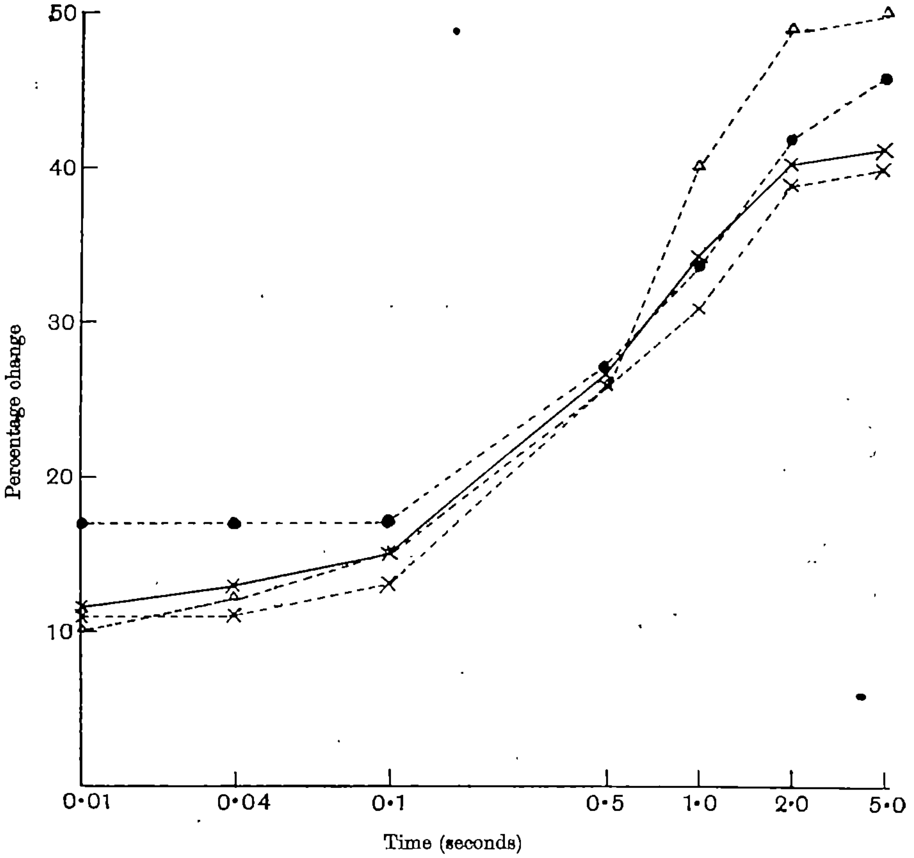

Fig. 2. ——— average curve; – – – – typical individual curves.

difficult to determine within 1 or 2 per cent of the total illumination. At these speeds subjects reported that in general they could see that a change had taken place long before they could see the change occurring. In fact the majority reported that they could never see the change taking place at the slowest speed, even with a 50 per cent change, which was the biggest possible with the apparatus used. In these cases, the threshold was taken at the point at which they could say accurately in which direction the change had occurred. This factor may perhaps explain the flattening which occurs in the curve at these speeds.

VI. DISCUSSION

The general shape of the curves obtained was similar to that obtained by Grindley (1936 *a*) for pressure. Fig. 3 shows the average curve for the ten subjects plotted on a scale comparable with that of Grindley's curves, i.e. with the abscissa representing the reciprocal of the rate of change. It will be seen that the curve is sigmoid, showing a tendency to flatten at both ends. The fact that the threshold increases with increase of the time occupied by a given change can be explained for light, as for pressure and touch, in terms of sensory adaptation. This has been discussed in the papers already referred to. In considering the exact form of the curves obtained for light, however, it is necessary to bear in mind the difference in the subjective reports and the probable difference in

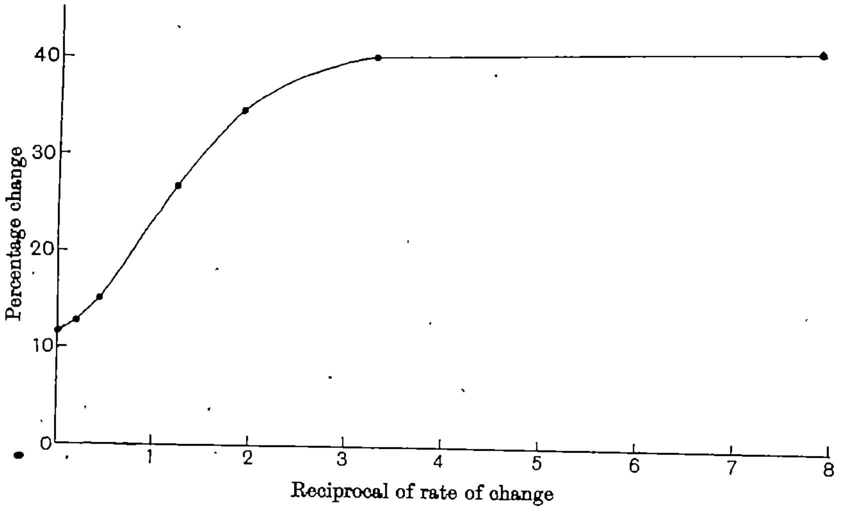

Fig. 3. Average curve.

the physiological basis of the experience of increasing illumination at the fast and at the slow rates of change.

At the slow rates of change the subjects reported, in general, that they judged that the light was brighter than it had been, rather than that they were conscious of a change occurring. Thus the report of an increase of illumination depended on a comparative judgement about two brightnesses, the basis of each of these experiences of brightness being, probably, a certain level of stimulation from the eye. If the accuracy with which comparative judgements of this kind can be made is relatively independent of the time elapsing between the two experiences, it is clear that the curve will flatten at its upper end.

At the rapid rates of change, on the other hand, the subjects reported that they were directly aware of this process of change, and in some cases, in fact, they were unable to say in which direction the change occurred. It is probable that the physiological basis of this is the 'on effect', i.e. the large burst of impulses occurring when a stimulus is rapidly changed (cf. Adrian, 1928). It is perhaps worth pointing out that the changes in the intensity of the sensation produced by a steady light (Broca and Sulzer, 1902, 1903), which have been attributed by Adrian (1928) to the 'on effect', suggest that in the human eye the 'on effect' may occupy a time of the order of 0·1 sec. It is therefore to be expected that, for changes much quicker than this, the time of change will have little influence on the size of the 'on effect'. The flattening of the curve at the lower end obtained in this experiment may be explained in this way.

VII. Summary

An attempt has been made to study the influence of the rate of change of brightness on the differential threshold for increased brightness. It is found that within a range of times of change of 0·01 sec. to 5·0 sec. the curve obtained is sigmoid. Possible explanations of this shape are formulated.

I wish to express my thanks to the Medical Research Council for a personal grant; and to Prof. F. C. Bartlett, Mr G. C. Grindley and Mr R. C. Oldfield for the opportunities they allowed me to discuss this problem with them.

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DISTRIBUTION OF PRACTICE AND SIZE OF MAZE PATTERN

BY THOMAS W. COOK

(*From Acadia University, Canada*)

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I. STATEMENT OF PROBLEM

IN an earlier study of factors conditioning the comparative economy of massed and distributed practice, the writer suggested that under certain conditions the emergence and clarification of insight are favoured by massing of repetitions, and the 'fixing' or 'setting' of more stable configurations by some form of distribution of effort. Taking this postulate as a working hypothesis, it follows that since one factor in the attainment of insight is the adjustment of the number of elements in the problem to the learner's 'span of apprehension', there should be a point in the difficulty scale at which massed procedure is most economical, with diminishing returns for (massed) effort expended with more (and possibly less) complex problems (1). The present experiment is designed to test the validity of this conclusion.

II. APPARATUS AND PROCEDURE

Apparatus suitable for our purpose must meet two requirements: it should (*a*) present a genuine problem, at least in the early stages of learning; and (*b*) permit increase or decrease in size without qualitative change in the nature of the problem. These demands are in some degree fulfilled by maze patterns of the standard T, U, or X type. For convenience in construction the mazes used in the present investigation were composed of U-shaped units.

Three pairs of U-type high-relief finger mazes were made by driving ⅞-in. staples into pine panels ((9), p. 429). The number of choice points or 'units' was 4 in mazes I and II, 8 in mazes III and IV, and 12 in mazes V and VI. The patterns of right and left turns in the 'true' paths were: maze I, *llrl*; maze II, *rllr*; maze III, *lrllrllr*; maze IV, *rrlrllrr*; maze V, *lrlrrllrlrrl*; maze VI, *rllrllrlrllr*. In massed procedure a subject had twenty trials in one sitting, with only sufficient time (5–10 sec.) between trials for an experimenter to replace the subject's finger in the starting position, and to give a ready signal. Distributed practice consisted of one trial a day for 20 days. In a few instances a subject missed an extra day, but a survey of these exceptions to the 24-hour interval shows no consistent effect from their occasional occurrence.

The subjects were members of the class in Introductory Psychology at Acadia University. Two groups learned each pair of mazes. Group A (6 women, 10 men) learned maze I by massed practice and maze II by distributed practice. Group B (6 women, 8 men) learned maze I by distributed and maze II by massed practice. Group C (7 women, 8 men) and Group D (6 women, 7 men) learned mazes III and IV, Group E (6 women, 8 men) and Group F (6 women, 8 men) learned mazes V and VI in an order similarly counterbalanced for massing and distribution. The order of presentation of the mazes was the same for all subjects: maze I, III, or V was always learned first and maze II, IV, or VI second. With few exceptions trials on the 'second' maze began 1 day after the completion of work on the first maze. Combining the results for each pair of mazes by averaging the group averages equates practice effects and maze and group differences for massing and distribution with a given complexity of pattern, but practice only for mazes of different size. For the latter the group differences were minimized by the number of subjects (28–30) learning each size of maze pattern, and pattern differences were lessened by making the 4, 8 and 12-unit patterns as nearly comparable as the writer's knowledge of the factors conditioning maze difficulty permitted.

Members of the class in Psychology of Learning (seniors and graduate students) conducted the experiments.[1] All observers were given pre-

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My thanks are tendered to these and to Miss Jessie Harding, Laboratory Assistant in Psychology at Acadia University.

liminary training and detailed instructions, and I have every reason to have confidence in the accuracy and uniformity of their work. To facilitate observation each group of subjects was divided into two subgroups, one composed of women and the other of men, and each subgroup was under the direction of an experimenter of the same sex. For massed practice two experimenters were present, for distributed practice only one experimenter. Errors were recorded from numbers on each panel and time taken with a stop-clock.

Before the experimental work was begun the writer explained to the class the nature of maze problems in general and U patterns in particular, illustrating the two-alley choice and the general direction of the goal by diagrams on the blackboard. The necessity of avoiding all thought of the patterns between trials was stressed. In the laboratory each subject was asked to read a typewritten copy of the following:

> You are to trace two mazes: one 20 trials in immediate succession; the other one trial a day for 20 days.
>
> Do *not* think of the experiment between trials. The aim of the experiment is to study the effect of time interval between practice periods on learning, and thinking about the maze pattern constitutes uncontrolled practice and might spoil the experiment.
>
> Notice that the general direction of the maze path is away from you.
>
> Do not hurry. Adopt an even, steady rate of movement. Avoiding errors is more important than initial speed.
>
> When you make an error retrace *only* to the point where you left the true path. Then go ahead: that is, take the turn (right or left) opposite to the one just made.
>
> Touch the maze with your forefinger only. Close the other three fingers and thumb in a 'fist'.
>
> The experimenter will say 'ready', 'GO'. *Wait for the word GO.*

After mastering these instructions the subject put on a pair of auto-goggles with cardboard eyepieces and learned a small practice maze. Next the blindfold was removed and the subject allowed to see the pattern and trace it again with his forefinger. The blindfold was then replaced and the experiment proper began. This preliminary training obviously lessened tension and confusion, and so familiarized subjects with the situation that retracing errors scarcely ever occurred.

III. GENERAL EXPERIMENTAL DATA

(1) *First trial*

A recent study has shown that when retracing is not permitted the number of errors and amount of time expended in the first trial with a visually perceived maze are proportional to the size of the pattern (2).

In the present experiment the same law holds for the error measure. The data presented in Table I indicate that first trial errors average 0·5 per cul-de-sac for the 4, the 8, and the 12 choice patterns. For time there is a slight *decrease* in relative difficulty with increasing size of pattern. For massed practice the 4, 8, and 12-unit mazes require on the average 3·9, 3·6, and 3·3 sec. per cul-de-sac (6); for distributed practice, 4·1, 4·0, and 3·2 sec. per cul-de-sac, in the first trial. It will be noted that 'massing' and 'distribution' give practically identical results, a fact also indicated in Fig. 1 by the common starting point of each pair of 'solid' and 'broken' curves. This should, of course, be expected, since in distributed practice the time interval was not inserted until after the first trial. However, the small size of the differences is a touchstone of the extent to which the number of subjects in each group and the method of combining the data from two mazes were effectual in equating individual variability and other 'chance' factors, for massed and distributed practice.

Table I. *Average errors and time in seconds required for tracing 4, 8 and 12 choice finger mazes*

Size of maze pattern (unit)	Average errors		Superiority of massed %	Errors per cul-de-sac		Average time		Superiority of distributed %	Time per cul-de-sac	
	Massed	Distributed		Massed	Distributed	Massed	Distributed		Massed	Distributed
					First trial					
4	1·94	1·94	—	0·49	0·49	15·4	16·1	—	3·8	4·1
8	4·0	4·0	—	0·50	0·50	28·6	31·6	—	3·6	4·0
12	6·0	5·9	—	0·50	0·49	39·3	38·3	—	3·3	3·2
					Trials 2–20					
4	5·0	9·9	98	1·2	2·5	118·6	115·9	3	29·6	29·0
8	30·3	38·7	28	3·8	4·8	314·7	279·4	13	39·3	35·0
12	59·3	64·9	10	4·9	5·4	440·9	366·6	20	36·7	30·5

(2) *Later trials*

The data for trials 2–20 (Table I and Fig. 1) may be summarized under three heads. Massed practice tends to have the advantage over distributed in (*a*) errors as contrasted with time, (*b*) simple maze patterns, (*c*) early trials.

(*a*) *Errors v. time.* The greater economy of massed practice for errors and distributed practice for time appears in the data presented in Table I. Fewer errors are made and more time is required with massed procedure for all three complexities of pattern. For the 4, 8, and 12-unit mazes the percentage superiority of massed practice in errors is 98, 28, and 10 per cent.; while with the time measure the advantage of distributed practice for these three sizes of pattern is 3, 13, and 20 per cent. Of the six measures only the first of the error percentages (98) has a satisfactory reliability, but the consistency of the trends increases their significance. Fig. 1 shows the same facts in graphic form. Wherever the error (left graph) curves differ the massed (solid line) is lower than the distributed (broken line); wherever a pair of time (right graph) curves diverge appreciably the distributed is nearer the base line.

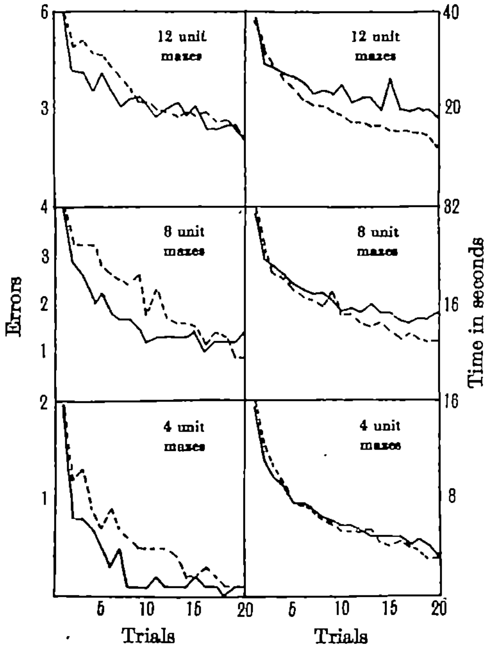

Fig. 1. Twenty trials massed and distributed practice with 4, 8 and 12-unit finger mazes.

——— Massed practice. - - - - - Distributed practice.

(*b*) *Size of pattern.* In Table I the percentage advantage of massed over distributed procedure in errors continuously decreases (98, 28, 10 per cent.) from the 4 to the 12 choice patterns; conversely, the superiority in speed of distributed practice increases with the size of the pattern (3, 13, 20 per cent.). The same trends may be distinguished in Fig. 1.[1] Reading up in the figure, the differences between the solid (massed) and broken (distributed) lines *decrease* for the left or error curves, and *increase* in the right or time graphs.

(*c*) *Place in the learning curve.* Fig. 1 shows that the superiority of massed practice by the error criterion (left graph) is wholly due to more rapid elimination of errors in early trials. After trial 19 for the 4 choice mazes, trial 15 for the 8 choice mazes, and trial 10 for the 12 choice mazes there are no significant differences between the error curves for massed and for distributed practice. On the other hand, the advantage of distributed practice in time appears only in the later trials. For the 4-unit patterns this superiority is very slight, but for the 8-unit and the 12-unit patterns distribution is increasingly more economical of time from trial 4 to trial 20.

These results are in harmony with and tend to confirm the hypotheses previously advanced (1).

IV. THE RELATION BETWEEN DIFFICULTY OF LEARNING AND SIZE OF MAZE PATTERN

Translating the data for all trials subsequent to the first (Table I) into numbers comparable to the 1, 2, 3 ratio of the 4, 8, and 12 choice patterns (by letting the number of errors or seconds with the 4-unit mazes equal 1, and obtaining corresponding values for the 8 and 12 choice patterns by dividing their average errors or average time for the 4-unit mazes), the error ratios for the three sizes of pattern are: massed practice, 1, 6, 11·9; distributed practice, 1, 3·9, 6·6. The increase in difficulty is much greater than the increase in complexity, more marked for massed than for distributed practice, and greater from the 4- to the 8-unit than from the 8- to the 12-unit patterns. For time the corresponding ratios for the 4, 8, and 12 choice mazes are 1, 2·7, and 3·7 with massed, and 1, 2·4, and 3·1 with distributed repetitions. Here the increase in difficulty is also greater than the increase in size (1, 2, 3), and for massing than distribution, but both trends are less marked than is the case with the error measure.

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[1] The curves for the three complexities of maze pattern are plotted on equal rectangles of graph paper, in order to make the relative differences between the curves for massing and distribution more comparable for the 4-, 8-, and 12-unit patterns.

V. THE TYPE OF ERRORS IN MASSED AND IN DISTRIBUTED PRACTICE

It was hoped that the experiment might show whether characteristic differences are to be found in the type of errors made under massed and distributed procedure. However, when the entrances into each blind are computed in percentages of the total errors for each maze, the net results reveal but one well-marked tendency and a few suggestions for further work.

(1) *First cul-de-sac*

Only in percentage of entrances into the first blind is there a clean-cut difference between massing and distribution of repetitions. With distributed practice, for all mazes except maze III (Table II), entrances into the initial cul-de-sac comprise a much larger percentage of the total errors than is the case with massed practice. The 'percentage superiority' of massed practice in first blind entrances is 75, 69, 87, 70, and 44 for mazes I, II, IV, V, and VI, while the only exception to the general rule is the 4 per cent. superiority for distributed practice in maze III. The average advantage of massing in percentage of first blind entrances for all size mazes is 56·3 per cent.[1]

Table II. *First cul-de-sac entrances as percentages of the total errors*

	I	II	III	IV	V	VI
Massed	6·1	14·3	5·0	2·3	3·0	4·5
Distributed	10·7	24·2	4·8	4·3	5·1	6·5
Percentage superiority	75	69	−4	87	70	44

(2) *Alternation and anticipation*

The development of tendencies to follow each forced right turn with a left turn and each forced left turn with a right (10), and to 'anticipate' choices one or more units ahead in the maze path (5, 7, 8) have been noted by several experimenters. In our data there are indications that both of these tendencies reach their maximum more rapidly with massed than with distributed repetitions. Thus in maze IV (*rrlrllrr*) a disproportionate number of errors (more than 12·5 per cent. of the total) in the final cul-de-sac might reasonably be attributed to a tendency to alternate right and left turns. Turning to the data, we find that the errors made in blind 8 (trials 2–20) are 15·2 per cent. of the total in distributed practice and 13·6 per cent. in massed practice. These percentages are greater than chance expectation but not reliably so. However, for trials 2–5, 6–10, 11–15, 16–20, we find that the percentages of the total errors made in the final cul-de-sac are 22·5, 9·7, 3·5, and 9·8 in massed practice, and 10·9, 13·9, 18·7, and 17·5 in distributed practice. These figures might be taken to indicate that the alternation tendency develops more rapidly but is eliminated more quickly in massed than in distributed practice.

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[1] In total number of entrances the advantage of massing is of course much greater.

Spragg has pointed out, however, that patterns such as were used in our experiment are not well adapted to distinguish 'alternation' and 'anticipation' from each other or from any other trends that might be operative (7). For that reason (although the piling up of errors in cul-de-sacs where the two tendencies might be expected to co-operate is marked in our data) it does not seem worth while to attempt a detailed analysis of the error distribution.[1] The immediate need is for adaptation to the problem of the methods used by Spragg in his investigation of anticipation, and extension of the work of Wingfield and Dennis on the relation of the alternation tendency to massing and distribution of repetitions (10).

(3) *Repetitive errors*

Elsewhere I have suggested that Lashley's and McGinnis' finding, that with massed practice a larger proportion of identical errors occur in successive trials than is the case with distributed practice, might be reversed with simpler mazes (1). One of the secondary aims of the present experiment was to obtain data relevant to this issue. However, several considerations intervened. In the first place, the problem is more complicated than at first appeared. It seems certain that repetitive errors may be due to any of a variety of factors, some of which (as 'alternation' and 'anticipation') develop during the learning. Computation of the gross number of identical and diverse errors made in learning mazes such as were used in the present experiment could not therefore be expected to yield much of value. What is needed is a study of the influence of massing and distribution of repetitions on the development of the different repetitive tendencies, under conditions that permit analysis of the distinct effect of each. Secondly, I can no longer agree that a higher percentage of repetitive errors need always signify the use of a less economical method of learning. "Persistence of errors through prolonged periods of practice" ((4), p. 294) could indeed be so interpreted, and indicate that greater flexibility and adaptability are characteristic of the mode of attack on a problem when trials are separated by a moderately long (as contrasted with a short) interval. But, on the other hand, one of the most striking manifestations of expanding insight is narrowing the range of overt response. Thus a cat who perceives the general location of a button in a puzzle box might well make more repetitive errors in early stages of learning than an animal that had not yet differentiated the situation to the same degree. Finally, Dennis and his co-workers, in a series of ingenious and careful experiments with rats, have shown that under certain conditions there is *a greater diversity* of response in massed than in distributed practice. Wingfield and Dennis are inclined (in direct contrast to Lashley and McGinnis) to attribute the 'superiority' of distributed over massed practice to "a stronger impulse toward the true pathway when only one trial is given. More trials lead to the taking of diverse pathways" ((10), p. 146). The findings of Dennis and his associates are of undoubted theoretical importance, but an attempt to give them the attention they deserve would exceed the scope of this paper. References to their earlier work will be found in the publications by Dennis (3) and by Wingfield and Dennis (10) cited.

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[1] The number of errors and the percentage of the total errors for each blind have been computed for groups of five trials and will be furnished on request.

VI. SUMMARY

Massing (5–10 sec. intervals) and distribution (1-day intervals) of 20 trials with Warden U-type finger mazes (subjects blindfolded) were compared for 4-, 8-, and 12-unit patterns. First trials errors and time were equal for the two distributions. For trials 2–20 massed practice tended to be more economical in total errors, in small mazes, and in early trials; conversely, distributed practice was superior in total time, in the larger maze patterns, and in later trials.

Errors made on the first trial were proportional to the size of the pattern; speed in the first trial decreased relatively to increase in size. With trials 2–20 the increase in relative difficulty was greater than the increase in size of pattern, for massed than for distributed practice, and for errors than for time.

Entrances into the first cul-de-sac were more frequent in distributed practice. Other tendencies for type of errors to be differentially affected by massing and distribution of repetitions were not sufficiently clear to merit detailed analysis.

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OSCILLATION

By W. H. ENTWISTLE

(*Queen's University, Belfast*)

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  (*a*) *Problems* (p. 313).
  (*b*) *Periodicity* (pp. 313–314).
  (*c*) *General and specific factors* (pp. 314–316).
II. *Description of tests* (p. 317).
III. *Procedure* (pp. 317–318).
IV. *Results* (pp. 318–326).
V. *Summary and conclusions* (p. 327).
References (p. 328).

I. INTRODUCTION

(*a*) *Problems*

THE findings here described are taken from a much wider piece of work on fatigue, practice and oscillation(1). But as the experiments were framed to throw as much light as possible on problems of periodicity, the appropriate results have been extracted for separate publication. Broadly speaking, they confirm the findings of Philpott(2) with regard to the 'general purposes' nature of elements in the work curve, while at the same time verifying the trough sequences of his theoretical curve(3).

(*b*) *Periodicity*

As we shall assume Philpott's theory of the curve, it will be as well briefly to outline his conceptions. He says the curve of output can be expressed as the sum of a series of geometric waves, i.e. waves such that distances from trough to trough are equal when plotted against the log of time. He suggests that there are many possible waves, all characterized by the fact that their logarithmic wave-lengths are whole number multiples of a given unit. Drawing up a table of hypothetical and empirical wave-lengths ((2), Table III), he shows that empirically observed wave-lengths appear to be scattered in much the same way as are highly factorizable numbers in the scale of natural numbers. Whence, he derives the notion that they measure L.C.M. or repeat cycles, and not simple waves as such. He does not deal with amplitudes, beyond noting the many times reported fact that they seem to increase towards the end of any given curve. In respect of phase, however, he makes the very important claim that there is constancy everywhere, any given wave being associated with a definite trough sequence. Finally, he claims to show that all possible trough sequences converge on a point $T_0 = 4{\cdot}076 \times 10^{-23}$ sec. from zero. Working back from these conceptions, he next attempts to construct a curve containing all the possible waves, thus embodying his notion of a 'Grand Total' curve. The fact that the appropriate amplitudes of the elementary waves are not known is a serious obstacle, but fortunately it is possible to obtain an approximation to such a curve in quite general terms.

The characteristic points in an actual work curve are fairly sharp troughs centred at given points. Their depths are immaterial for present purposes as long as they are marked enough to be visible. Assume now all the possible whole number periods, and inquire what should be happening at a point say 12 log units from the universal common trough point T_0. Clearly, we shall there find the centres of troughs of waves of periods, 1, 2, 3, 4, 6 and 12 respectively, those and no others. On the other hand, at point 17, two, and only two troughs will be centred, namely of waves 1 and 17. So that if all the possible waves are present we are more likely to find a trough in the resulting curve at point 12 than at point 17.

Put generally, the possibility or probability of a trough occurring at any given point in such a curve is approximately given by merely factorizing the time value concerned. Making the necessary calculations, Philpott has shown (3) that the most highly factorizable points occur at:

38 49 63½ 102 130 ... Zone *A* 400 600 900 ... Zone *B*
464 646

sec. from zero. These he claims are actually the points at which troughs are most likely to appear in a statistically heavy curve, i.e. one based on some hundreds of records. As so stated, the theoretical conception is perhaps a little too simple. There are several difficulties, in particular that of appropriate major cycles. These, however, we may leave until our own results are discussed.

(c) *General and specific factors*

Another group of problems is related to what happens when two tasks —*A* and *B*—are sandwiched together. Suppose the subjects begin with, say, addition, thereafter at a given moment changing (without stopping) to the process of subtraction. There will be three possibilities as far as waves are concerned, namely:

(1) Given waves may run through both parts of the double task, being common to both.

(2) Waves may stop at the point of change, being peculiar to task *A*.

(3) Waves may begin at the point of change, being peculiar to task *B*.

Philpott suggests that all three things happen ((2), pp. 75 *et seq.*) but in special fashion. It is not that certain definite waves are always peculiar (or specific) to each of the tasks *A* and *B*, or that others are as definitely general (or common) to both. He says we may assume given waves to appear sometimes in the one task, sometimes in the other, in both, or in neither, it being a matter of chance.

Experimentally, waves passing the point of change will be found in curves showing the sandwiched work in full, i.e. including all the work, whether done at task *A* or task *B*. These he has called Full curves. If the *A* portions of work are cut out entirely, the time intervals being renumbered to make the points of change zero time, curves are obtained showing output in task *B* alone. These have been called Extracted curves.

Using Full, Extracted, and Straight curves,[1] Philpott sets out the possibilities as follows:

(1) If only general waves exist, none being specific or common, then all Full and Straight curves should resemble one another, being all of one general pattern. On the other hand, Extracted curves should be meaningless (i.e. non-periodic) except in so far as they may be interpreted as the end parts of corresponding Full curves.

---

[1] A Straight curve is derived from an ordinary or straightforward experiment where there is no sandwiching. Full and Extracted curves are named from the *B* tasks concerned. Thus, for example, suppose we are working with all the possible *A*/*B* combinations of tasks *P*, *Q*, *R* and *S*. They are:

P tests	...	*Q*/*P*	*R*/*P*	*S*/*P*
Q tests	...	*P*/*Q*	*R*/*Q*	*S*/*Q*
R tests	...	*P*/*R*	*Q*/*R*	*S*/*R*
S tests	...	*P*/*S*	*Q*/*S*	*R*/*S*

If we extract the *P* portion of any of the *P* combinations then, quite naturally, we are dealing with an Extracted *P* curve. To be consistent, the Full curve of say the *Q*/*P* combination is called the Full curve of *P* when sandwiched with *Q* as the *A* task. (We shall not present any of these elementary curves.) The sum of the three possible Extracted *P* curves is the Total Extracted *P* curve. Similarly, we get Total Full curves. Finally, we get Grand Total Extracted (or Full) curves by summing the four contributory total curves concerned.

(2) If there are only specific waves, individual Extracted curves should vary with the task performed, resembling corresponding Straight curves in all respects. Full curves should be meaningless, save in the portion occupied by the *A* tasks alone.

(3) If there are general and specific waves existing side by side, individual Extracted curves will be characteristic in outline, but different from one another. Individual straight curves will also be characteristic of the task, but rather less so, differing from corresponding Extracted curves in respect of any general waves they may contain. Full curves, should, on the other hand, be all alike, giving each a picture of the complex set up by the waves assumed to appear universally in every work curve.

(4) If common elements are present as well as specific and general ones, Extracted curves should resemble one another somewhat by pairs or other small groupings. Individual Full curves should at the same time tend to lose their resemblance to one another, since individually they will contain waves common to given groups of tasks.

(5) If waves are neither general nor specific, all being common to two or more tasks, but not to all, then the Grand Total Full, Extracted and Straight curves should tend to resemble one another. Individual Full, Extracted, and Straight curves should, however, differ, and be constant in their differences.[1] (This last point of constancy serves to distinguish this possible case from the next.)

(6) Finally there is the conception of a pool of general purposes wave-lengths, emergence of any particular wave being determined by factors other than those of the task, the subject and so on; factors work in such a way that emergence seems to be chance-determined, given waves coming sometimes in an *A* task, sometimes in a *B* task, sometimes in both, or in neither. In such a case, Grand Total Full, Extracted, and Straight curves will come to resemble one another if based upon a sufficiently large number of records, since it will be only a matter of time for all the elementary waves to appear, whether on the Full or Extracted side. In similar fashion, individual Full, Extracted, and Straight curves will come to resemble one another if enough records are taken. There will be no differences anywhere, save those of chance sampling among the elementary waves concerned.

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[1] *Small* general, specific or individual differences may of course remain masked in experiments of the kind. Exhaustive proof of their absence would be difficult ((3), p. 242).

II. DESCRIPTION OF TESTS

The subjects for this present series of experiments were girls selected from Classes I to XII of an L.C.C. Central School. Their ages varied from 11 years 10 months to 16 years 7 months, the average age being 14 years 9 months. About 60 girls actually attended; of these 50 did not miss a day throughout the whole 43 days, and only their results were taken into account.

Sheets of figures of the Kraepelin type were used. The size of the sheet (10 × 21 in.) was such that it could be conveniently placed on the desks and yet contain the whole 20 min. work. Each sheet contained 130 lines of printed figures arranged in 20 vertical columns with a space of ½ in. between the columns. Light lines were ruled across the sheet under each horizontal row of figures in order to guide the subjects in putting down their answers. Each line was numbered in heavy, black print (1–130), and the spaces between the columns were numbered at the top (0–20). Columns were ruled at the right-hand side for (*a*) Gross totals, (*b*) Errors, (*c*) Correct sums for each line. There were five different tests, namely Addition, Subtraction, Multiplication, Division and a 'Mixed' test which consisted of sandwiched tasks.

(1) In the *Addition test* the subject worked across the page. The answer to the first addition sum was placed on line 1 under column 1. When the sum of the two numbers was greater than 9, the left-hand digit was omitted in the answer. Each printed figure was added to the immediately preceding printed figures on the left, the answer to the second addition being placed in column 2, and so on, until a signal was heard. A bell was rung every 10 sec. at each such signal, and the subjects began a fresh line of work.

(2) The *Subtraction test* was similarly worked across the sheet. When the printed figure on the right was smaller than the one immediately preceding it on the left the subtraction process was easy. When it was greater, as for example 9 as compared with 7, the subjects were told to add 10 to the smaller number saying to themselves, "Nine from seventeen leaves eight."

(3) The *Multiplication test* was worked in a similar manner. Here the subjects multiplied the numbers by pairs. The left-hand digit was again omitted when the product was greater than 9.

(4) The *Division test* was probably the most difficult. The subjects worked rapidly when the divisor was smaller than the dividend. If the divisor were larger, as for example 9 as compared with 7, the girls worked mentally as follows: "Nine into seven won't go, nine into seventy equals 7 and 7 over." They put down the quotients only and ignored the remainders.

(5) The *Mixed test* consisted of a short *A* task followed by a long *B* task. The subjects began with, say, addition, and continued for a dozen lines or so until they came to a red line drawn across the page, and labelled with a given sign, say minus. They thereupon switched over from one process to the next.

III. PROCEDURE

The experiments began on Monday, 6 October 1931, and continued till 11 December 1931. There were 43 working days in all. Saturdays and Sundays were not used, and in addition there were two days for mid-term break in the third week. Each day's work lasted 20 min. and took place at the end of the morning session.

The girls were asked to sit in comfortable positions, keeping well away from their neighbours' elbows and placing the test sheet so that it need not be moved during the experiment. An electric bell was rung automatically every 10 sec. The first signal with the bell was taken as a warning. All eyes were on the experimenter who said, 'Ready!' At the next peal of the bell the experimenter said, 'Now!', and the work began. At the next peal of the bell the girls switched to the next line of work and so continued for the full 20 min.

The girls did not know the exact line at which they were to stop. In spite of this, end spurt was not altogether eliminated. The explanation is perhaps, that the lines were numbered and the subjects had an idea as to when the end was coming. However, some were stopped at line 126, others at line 127, and so on. In the mixed test, however, the subjects always continued till line 130, although only the first 120 lines of work (20 min.) were included in the results.

The procedure in the sandwiched work was slightly different. The subjects had two tasks in one sitting, namely a short *A* task and a long *B* task. The change-over took place sometimes after 120 sec. of task *A*, sometimes after 130 sec. and so on, up to 170 sec. after which time all had changed. The tasks were sandwiched cyclically. By allotting the tasks in cyclic order both as regards change points and as regards tasks, an even distribution was obtained amongst the children. There were 12 arrangements of *A* tasks taken two at a time, Addition—Subtraction, Subtraction—Addition, Addition—Multiplication, and so on. The 6 change points were labelled *A*—*F*, and the 12 combinations of tasks 1—12. Thus 1*A* at the top of the paper meant to the girls concerned "you begin with addition, change after 12 lines, and finish with a long task of subtraction". Matters were not, however, left to the children to interpret. A red line was drawn across each test sheet at a given number of rows from the top. This acted as a signal to change from task *A* to task *B*. The papers were prepared beforehand, and the signs +, −, × and ÷ put at appropriate places. There was thus no possible room for doubt. These mixed or sandwiched tasks proceeded as smoothly as did the others.

Visitors were struck by the application and interest displayed by the girls in this somewhat dull and mechanical arithmetical work. There was absence of coughing, shuffling of feet, and restlessness. Even towards the 43rd day, none of these signs was noticeable.

IV. RESULTS

We have to enquire whether the curves are periodic, whether they conform to Philpott's analysis in wave-length and phase, and whether resemblances between Full, Extracted, and Straight curves are sufficient to justify his contention that waves are of general purposes nature, appearing indiscriminately in all three types of curve.

Broadly speaking, an answer can be given to the last question at once. Whether Full, Extracted, or Straight, the curves tend to resemble one another. Moreover, the Grand Total curves (Fig. 4) are even more alike than are the various contributory curves of Figs. 1–3. On the whole, therefore, we are compelled to support him in saying that alternative (VI) of p. 316 is the most likely.

Our judgement here has been based on visual inspection of the curves. Philpott himself went further. He devised the method of 'shapes' ((2), pp. 77 *et seq.*), thus obtaining a statistical measure of the extent to which the curves in any given set resemble one another. In verifying his analysis, we might therefore have applied the same method, measuring degrees of resemblance between the four curves of Fig. 1, comparing with similar measures from Figs. 2 and 3, and finally with those for the Grand Total curves of Fig. 4. On the other hand, the method is laborious, and since this present work was but part of a larger research, there was no

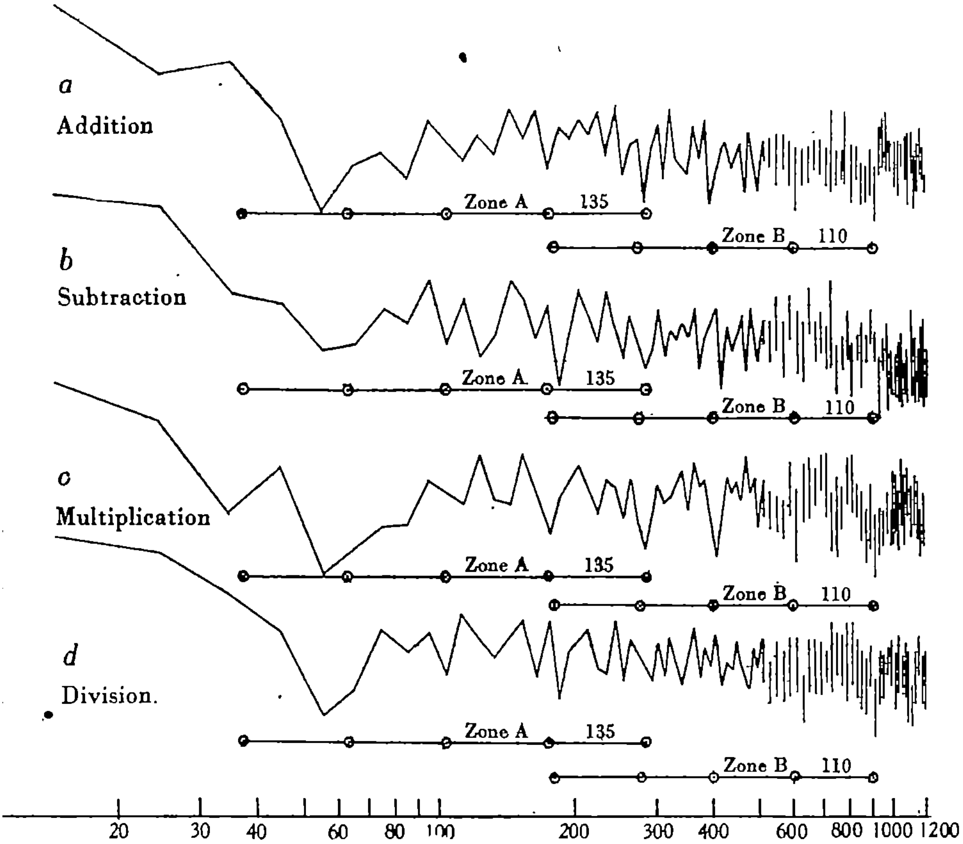

Fig. 1. Straight curves.

time available. In any case, we can fall back on Philpott's theoretical curve, for if our curves resemble one another by reason of the fact that they all more or less approximate to the theoretical system, it follows that our case will be almost strong enough, even although based on visual inspection.

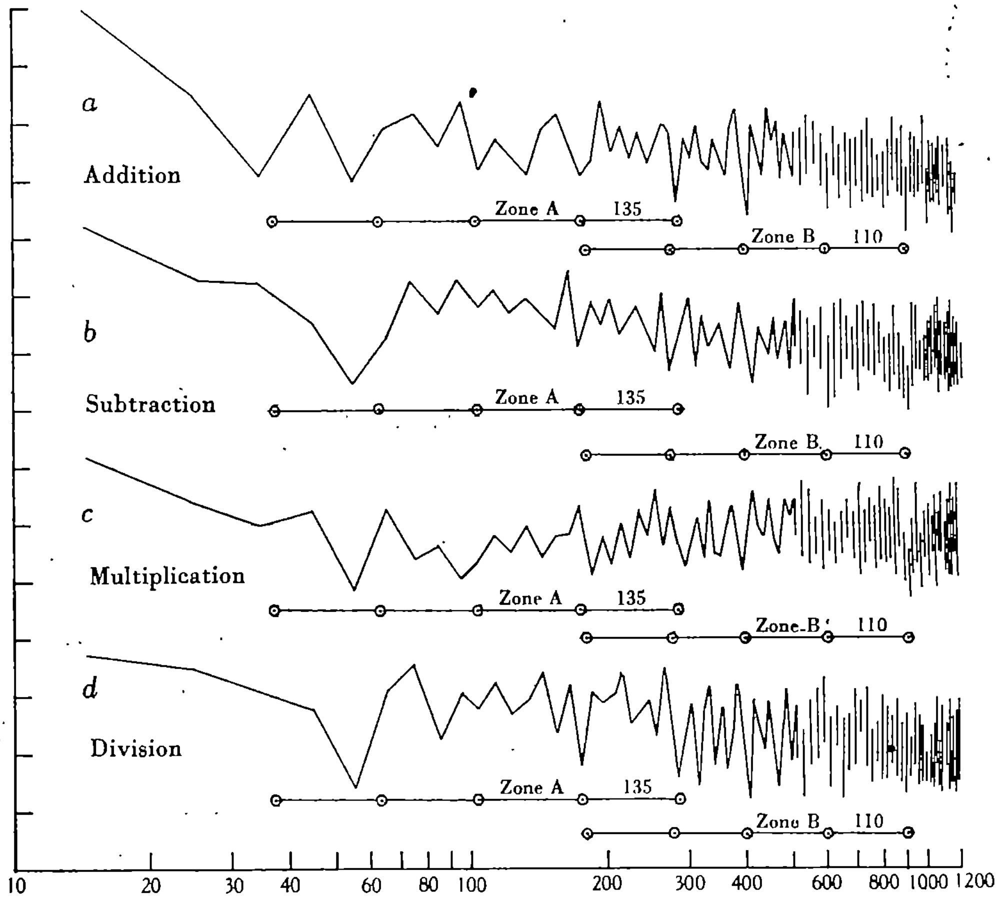

Fig. 2. Full curves.

The argument is simple. If degree of resemblance between curves increases with the number of records contained in them, it must be because they are approaching a standard outline. This is the only logical conclusion, and Philpott did not fail to arrive at it. Moreover, as indicated in his monograph, it was when puzzling over the nature of a standard curve of the kind that he began seriously to think of standard wave-lengths, and standard trough sequences. So that from experimental results of the same kind as our own he proceeded to his general hypothesis, and finally to the construction of a theoretical 'grand total' curve that should be of 'standard outline'.

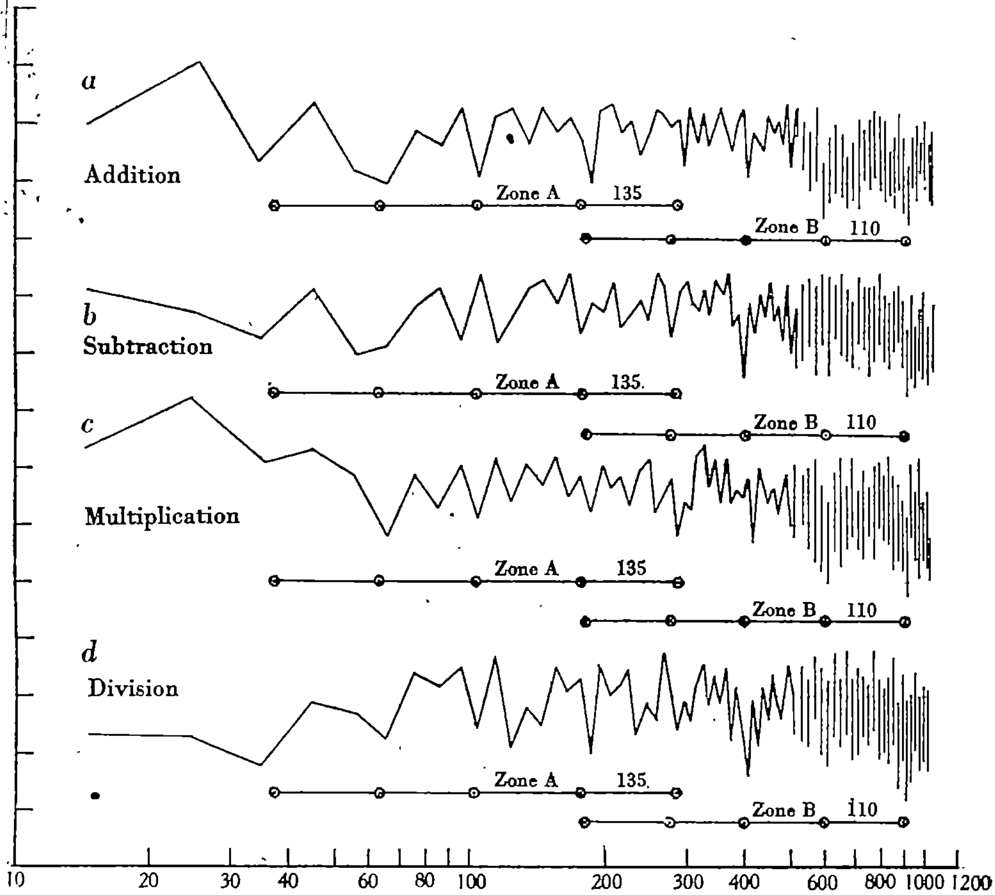

Fig. 3. Extracted curves.

With that curve before us, we may cut short some of the steps in the analysis. If our Full, Extracted, and Straight curves have troughs at his 'most probable' points, we shall be justified in accepting them as evidence in favour of his conceptions, thus answering all three questions raised at the beginning of this section.

Marks made under the various curves of Figs. 1–4 indicate troughs for a cycle of 135 log units period in zone *A*, and 110 log units in zone *B*.

The said troughs fall at:

39 (49) 63½ (81) 104 (133) 172 – Zone *A*
178 267 400 600 900 Zone *B*

seconds respectively, bracketed values in zone *A* indicating trough points required to halve the wave-length (making it 67½ log units); this makes clear a relationship with the figures quoted on p. 314 above.[1]

We have chosen the double wave-length because, although there are signs of the minor or 67½ unit cycle, the major pulses are of twice that period. In fact, some of them are fourfold. In Philpott's terminology, our pulses are usually of A_2 or A_4 orders of size, where an A_1 pulse is of anything between, say, 54 and 75 log units in duration.

Putting these details on one side, and looking merely at broad characteristics, there is no doubt that curves tend to have troughs at the given points. There are slight divergences here and there, but on the whole, conformity is the rule. Agreement is most marked perhaps in zone *B*. In most regular fashion, curve after curve has depressions in the near neighbourhood of 270, 400, 600, 900 sec. The rhythm is, in fact, the only one that could reasonably be suggested.

The findings are of special significance, as far as zone *B* is concerned, for Philpott was unable to verify his conceptions by an appeal to actual curves. His records were for the most part short, so that although he had about 700 from which to obtain a Grand Total curve confirming his notions of zone *A*, he had only 25 long enough to cover zone *B*. As it happens, the curve so obtained was of the postulated pattern, but with so weak a backing that the result might well have been otherwise. Our present curves are much stronger. There are about 400 records in the Grand Total Full, and Extracted, curves respectively, and over 1600 in the corresponding Straight curve. Conclusions based on them are thus likely to be significant.

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[1] The curves were drawn before Philpott's theoretical curve was published(3), and the markings under them really throw back to his earlier analysis(2).

He first enquired why so many different curves have a pulsation in zone *A* of between say 54 and 75 log units period, 67½ being an average kind of measure ((2), p. 55 *et seq.*). Tabulating (Table IX), he showed that most of the commonly occurring cycles of that order of size should—by hypothesis—come to a common trough at 63½ sec. There will be there centred a zone of interference, or 'beating', the mean period for a short distance on either side of the main trough being of course equal to the mean of the various periods concerned; hence the emphasis on 67½ log units as an approximate wave-length in that zone.

But in a footnote to p. 59 he further remarks that analogous zones of interference may be centred on common troughs sometimes at 38, sometimes at 49, at 102 sec., and so on. So that if enough elementary waves are present to set up all or most of these common troughs in one and the same curve, we shall arrive again at a rough 67½ unit cycle, for those other common trough points are nearly enough at the points mentioned above for a simple pulsation of that period. We thus get an alternative notion of what may be happening in a sufficiently complex curve.

Finally, of course, in his theoretical curve, Philpott merely states the various trough points, showing quite clearly that they are determined not by interference in the neighbourhood of just one common trough, but by the interaction of many waves. In effect, the footnote of the monograph becomes the main conception of his further article.

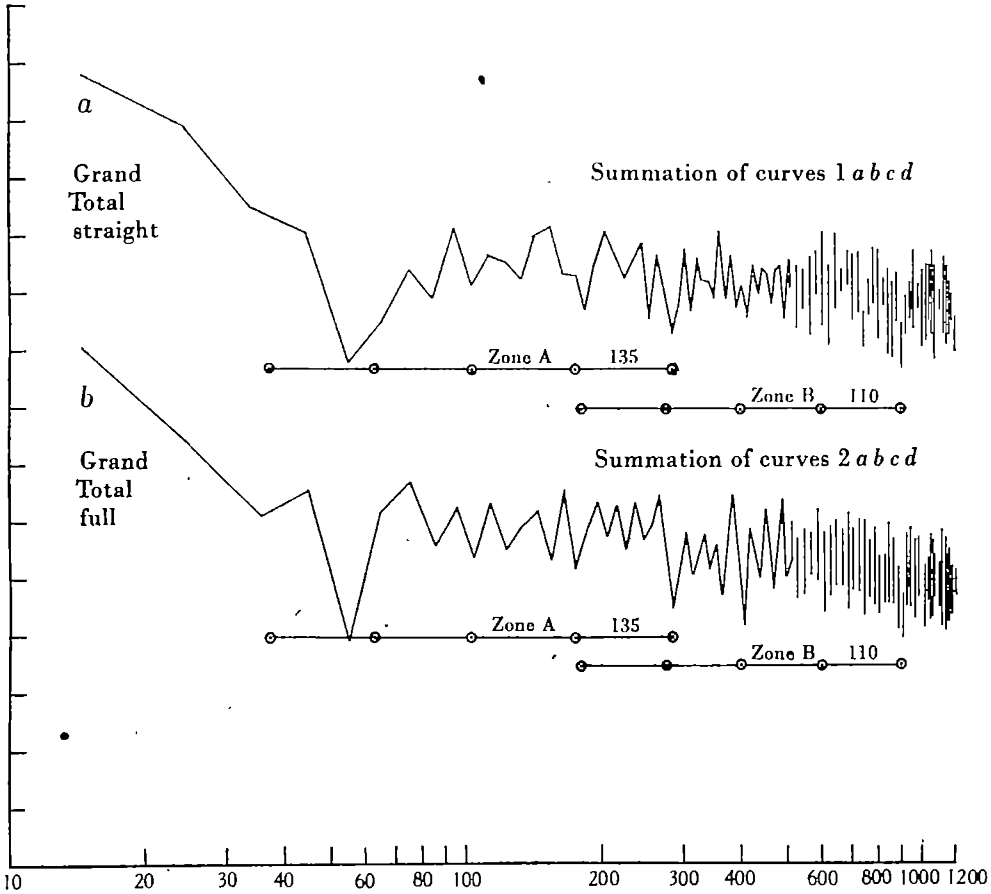

Fig. 4 (*a*), (*b*). Grand Total curves.

Curiously enough, it is in zone *A* that the most important of the apparent discrepancies is found, and that not in respect of a minor trough, but of the key trough itself, namely the one expected to fall at

63½ sec.[1] In terms of our rather coarse time measures, it should appear as a depression in the 60–70 sec. interval. (All curves are of work done per 10 sec.) It is true that the theoretical analysis is justified to the extent that a marked trough usually appears in the near neighbourhood (it need not have done), but in all Full and Straight curves it tends to

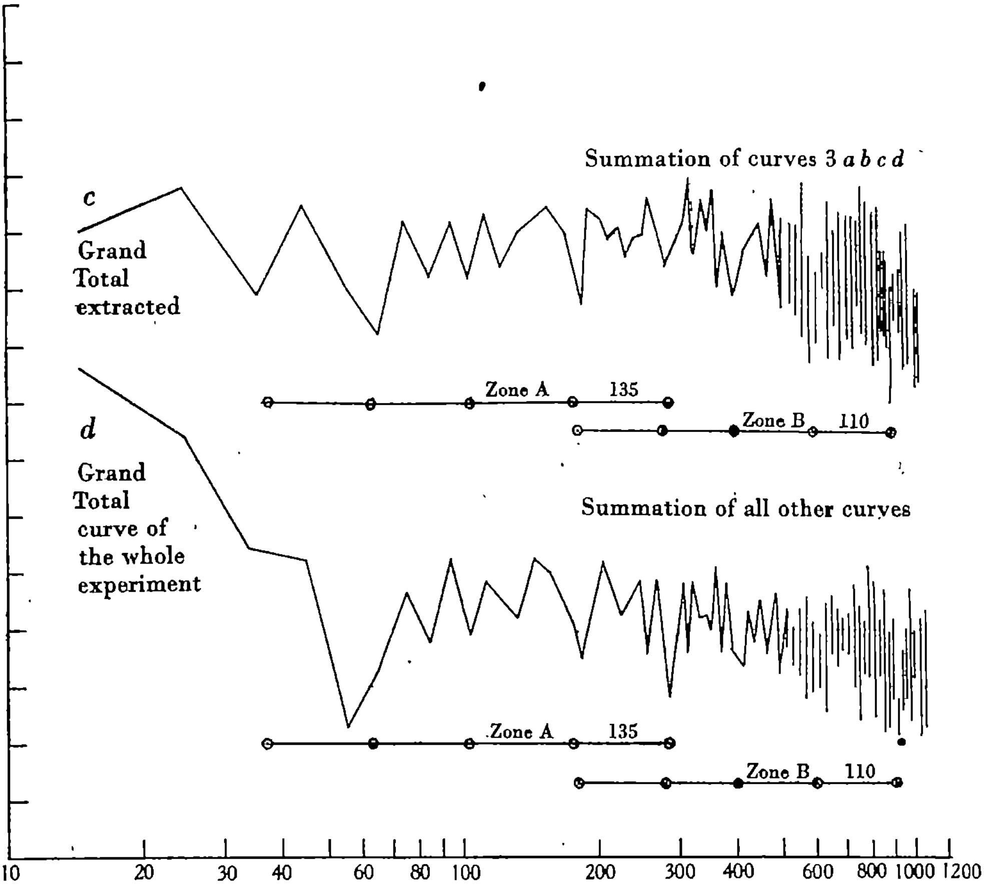

Fig. 4 (*c*), (*d*). Grand Total curves.

come in the interval next before the one expected, namely in that for work done between the 50th and 60th seconds.

A possible explanation of such cases would be that wave-length

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[1] Philpott has since discussed this point at length (3), for even in the theoretical curve the 63½ sec. trough, although present, is of no great depth. Knowing the method of construction, one can see why it should be shallow in the theoretical curve. Arguments in the text above apply, of course, to the empirical results.

110 dominates the whole curve, and not merely zone *B*. For if continued back from that zone, it would have a trough in the 50–60 sec. interval. In other words, our curves might be characterized by a simple cycle, rather than by composite or hybrid pulses in the two zones. Against such a notion is of course the theoretical demand that we ought to find composite cycles because of weight and complexity in the material.

A second explanation is that the apparent discrepancy may be due to faulty technique. It will be remembered that the subjects were mentally alert at the first peal of the bell, given 10 sec. before the formal time of commencement. It is quite possible that they actually 'set' themselves at that first peal (or immediately afterwards), thus starting waves that ran on into the time devoted to the task proper. If that were so, a trough actually engendered 63½ sec. after the moment of 'set' would really fall nearer 53½ sec. on our scale of time. It does not follow that it need be a full 10 sec. in error. But a backward shift of anything over 3½ sec. would obviously bring the trough centre into the interval next preceding the nominal one.

The 63½ sec. trough would be quite easy to shift in this sense. From Philpott's findings, both theoretical and empirical, it is deep and narrow. A shift of but 5 sec. would bring its centre to 58½ sec., while since it is so narrow, very little of it need overlap into the interval next following. Incidentally, certain other theoretically expected troughs would be more difficult to shift, being wider, and differently placed. Thus, for example, the points 38 and 49 sec. are near the top end of the intervals concerned, and a backward shift of 5 or 6 sec. to troughs there centred would not make much difference to them. So that assuming an experimental error of the kind, we might well find some troughs at the theoretically expected points, even if others were shifted. And, generally speaking, it is only the 63½ sec. trough that is at all markedly in question.

The foregoing argument with regard to preliminary drill does not apply in the case of Extracted curves, and we thus have a criterion to which appeal may be made. The children changed from task *A* to task *B* at given points, and those points are taken to be zero time for the Extracted curves concerned. So that even if disturbances were introduced by preliminary drill, they could only apply to the corresponding Full and Straight curves. The trough theoretically expected to fall 63½ sec. *after the point of change* ought to fall there in the Extracted curves, and that according to our present time scale.[1]

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[1] This explanation has been criticized by D. K. Grewal(4). She says that if waves begin at the preliminary signal the effect will be to make a double 'sandwich' of the experi-

It is true that the Extracted curves for addition and subtraction (*A* and *B* of Fig. 3) are somewhat depressed in the 50–60 sec. interval, but those for multiplication and division show definite troughs in the one next following, as does the Grand Total Extracted curve (Fig. 4 *c*). The apparent discrepancy in Full and Straight curves may thus quite possibly be due to the method of experiment. Fortunately, this is the main difficulty as far as zone *A* is concerned. For the rest, we may summarize by saying that all curves, whether Full, Extracted or Straight, tend to conform to the zone *A* analysis.

There are one or two further points to which reference may be made. One relates to the appropriate major cycle in zone *A*. The theoretical curve here fails us. It is probably clear enough as far as the minor or *A* cycle is concerned, but by slightly altering its constitution it can be made to take on A_2, A_3 or A_4 characteristics at will—the underlying A_1[1] pulsation persisting of course throughout. As we do not know which variant to accept, the problem is left to be settled experimentally. Philpott's Grand Total curve was of A_2/A_4 pattern. Generally speaking, our own curves agree.

Another problem is that of deciding between the two sequences given for zone *B* on p. 314 above. In the theoretical curve they coexist, the 400, 600, 900 sequence being dominant. That same series is also dominant in our own curves. Philpott's own curves were in the main not long enough to enable him to decide the point. Although his one curve also gave that sequence, some of his others of 750 sec. length seemed to lay more emphasis on the 464, 646, 900 sec. troughs. Generally speaking, the two sequences are perhaps best taken as alternatives, both being likely, the 400, 600, 900 sequence rather more so than the other.

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ment, giving another set of Full and Extracted waves. In detail, one set of waves should run through from the drill (call it task *D*) into tasks *A* and *B*. A second set (our present Full waves) should then begin, being Full as far as tasks *A* and *B* are concerned, but Extracted with reference to task *D*. Similarly, of course, a new set should begin at the point of change from task *A* to task *B*. If, therefore, we claim that our present Extracted curves are normal, she suggests that by the same argument, our present Full and Straight curves ought also to be so. It is a neat analysis, and she may be right.

[1] An A_1 pulse has a duration of anything between say 54 and 75 log units. A_2, A_3, A_4, ... pulses are two, three or four ... times the corresponding A_1 measure. In analogous fashion, B_1 pulses in zone *B* are at periods between 75 and 120 log units with corresponding B_2, B_3, ... measures. The dots made under our various curves (Figs. 1–4) indicate pulses of A_2 and *B* dimensions respectively.

V. SUMMARY AND CONCLUSIONS

The general aim of this paper has been to discuss certain problems of oscillation or fluctuations of attention, in particular those of (*a*) periodicity, (*b*) possible general and specific elements among the waves, and (*c*) possible trough sequences.

The tests were of addition, subtraction, multiplication and division, worked on modified Kraepelin sheets. The method was either that of the ordinary work curve, or that of the sandwiched experiment, the subjects beginning with some one of the tasks (*A*), thereafter changing at a given signal to another task (*B*).

Ordinary work curves have been labelled 'Straight'. Those showing the full course of a sandwiched experiment have been labelled 'Full'. Those of results from *B* tasks in a sandwich (ignoring the *A* portions, and renumbering the time line from the point of change as zero) have been labelled 'Extracted'.

The findings are that all three kinds of curve come to resemble one another more and more as the number of experimental records is increased.

We thus confirm Philpott's findings in the matter, and presumably his conclusions, namely that (*a*) the curves are periodic, and (*b*) that the contributory or elementary waves concerned are neither general nor specific, but of general purposes utility, appearing sometimes in *A* tasks, sometimes in *B* tasks, in both, or in neither—all on what seems to be a basis of random chance. It is as though we have a pool of possible waves from which we take a random handful at the beginning of every task, whether it is of *A* or of *B* nature in the sandwich.

Our various curves have been marked to indicate dominant troughs in zones *A* and *B* of Philpott's analysis. (The symbols *A* and *B* as used here have no relation to those of the sandwiched experiment.) The said markings show that the curves approximate very closely to his system. The trough expected to fall at 63½ sec. is sometimes replaced by one at approximately 55 sec. It is suggested that this may be due to experimental error, for it is more characteristic of Full and Straight curves than of Extracted, the latter curves—by definition—being presumably free from the error suggested.

Summing up, both the empirical findings and the theoretical analysis of our curves tend to support the main conceptions of Philpott. There is still a vast field of research to be opened up. This paper has touched merely the fringe.

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(*Manuscript received* 21 *March* 1936)

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CONTRIBUTORS are asked in future, when submitting papers for publication in the *British Journal of Psychology*, to observe the following requirements, in order to minimize the cost of publication:

(1) Papers must be clearly and legibly typed, on reasonably thick paper, with adequate margins and numbered pages. Typing must be double-spaced.

(2) The material must be divided into suitable sections, with headings and subheadings; and a list of these given at the beginning of the paper. Whenever possible, a summary should be given at the end of the paper.

(3) *Tables and figures* must be clearly and comprehensibly typed or written, with the figures placed neatly in vertical columns. The author should check all figures before submitting the paper, since arithmetical mistakes in tables of figures are frequent.

(4) *Diagrams* and *drawings* of every kind should be drawn in black ink only, sufficiently clearly and accurately to obviate the necessity of redrawing by a draughtsman. The lettering and numbering should be in pencil, but must be legible and comprehensible. Diagrams can always be reduced in size for reproduction, but it should be noted that the thickness of lines and spaces between lines is then proportionately reduced, and allowance for this should be made in the original.

Diagrams must appear on separate sheets of paper, and not be included in the body of the text. Photographs should be submitted only when absolutely necessary.

(5) The author should check titles, table headings, numbering and lettering of series, parts, etc., which are mentioned in more than one place in the text; thus, to refer in one place in the text to Series II (*a*) and in another to series 2 *a*, may cause much confusion, and is always queried by the proof reader.

(6) *Formulae* must be written or typed very carefully and clearly, especially when they consist of Greek lettering, and when they contain subscripts.

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must be checked, whenever possible against the originals, and given in complete form, as follows:

(1) Hazlitt, V. (1931). "Modern trends in infant psychology." *Brit. J. educ. Psychol.* I, 119.

(2) Shand, A. F. (1914). *The Foundations of Character.* London, Macmillan.

In references, the *British Journal of Psychology* is to be referred to as This *Journal.*

(8) *Footnotes* must be as brief and infrequent as possible, and should be given at the bottom of the page to which they refer, not in a list at the end of the paper.

(9) *Alterations* in the *proof,* apart from corrections of printer's errors, should be kept at a minimum. The insertion of paragraphs of new material is particularly troublesome to the printer.

Blocks of diagrams and drawings, once prepared, **cannot** be altered in the proof; thus any change necessitates the preparation of a completely new block.

Should papers be submitted which are notably deficient in these respects, they will in future be returned to the authors for correction before any decision as to publication has been made.

XI^e CONGRÈS INTERNATIONAL DE PSYCHOLOGIE

LE Comité d'organisation espagnol a dû, en raison des circonstances, renoncer à la réunion à Madrid, en 1937, du XI^e Congrès international de Psychologie.

Dès qu'il en fut officiellement avisé, le secrétaire permanent des Congrès, le Prof. Ed. Claparède, s'adressa au Prof. Piéron pour lui demander s'il accepterait éventuellement d'organiser le Congrès à Paris en 1937. M. Piéron ayant répondu affirmativement, mais en demandant une décision ferme avant le 31 octobre, date limite pour l'inscription des Congrès internationaux prévus à Paris au cours de l'Exposition de 1937, le Comité exécutif, désigné pour prendre les décisions d'urgence, put seul être consulté. La décision de la majorité des membres du Comité a été de réunir en 1937 le XI^e Congrès à Paris.

Un Comité d'organisation a été constitué, et une circulaire sera prochainement adressée, donnant tous renseignements utiles, le programme restant, dans les grandes lignes, celui qui avait été établi par le Comité espagnol.

La date est dès maintenant fixée. Le XI^e Congrès international de Psychologie se tiendra à Paris du 25 au 31 Juillet 1937, sous la présidence d'honneur du Prof. Pierre Janet.

Le Président du Comité d'organisation est M. Henri Piéron, le secrétaire général, M. I. Meyerson. Le bureau du Congrès est situé au Laboratoire de Psychologie de la Sorbonne, Paris 5^e.

PUBLICATIONS RECENTLY RECEIVED

Elements of Social Psychology. By HERBERT GURNEE. New York: Farrar and Rinehart. 1936. Pp. xi+467. \$2.50.

This is a text-book. Every chapter is followed by questions which the earnest student can endeavour to answer; the general tone is that of the teacher imparting information—naturally with somewhat frequent confessions of ignorance—and there are recurrent bibliographies. Social psychology is defined at the outset as the science of the activity or behaviour of the individual in so far as it is the consequence of stimulation by other human beings, and consequently the author can easily follow the example of many other writers on this subject and put in very nearly anything he likes. We therefore have studies of learning, motives, emotions, temperament and personality, development of language, suggestion, misconduct, aesthetic and religious activity. To all of them the word 'social' is prefixed and that puts the matter right. In addition there are two sections, one dealing with 'attitudes' within and towards social groups, and the other specifically with group behaviour. There are many illustrations and diagrams, some original, some taken from a variety of sources. In many ways the book is well done. Dr Gurnee shows an excellent knowledge of similar literature and a catholic sympathy. Anybody reading the book will learn much about a multiplicity of pieces of research. But maybe many people will wonder whether social psychology has as yet reached the stage in which instructional text-books can or should be written. It may be that what are needed are the clearer focusing of specific problems, the settlement of questions of technique, and the stimulation of directed research.

Conduct and Ability. By FRANCES BANKS. London: Methuen and Co., Ltd. 1936. Pp. xi+399. 10*s*. 6*d*. net.

Psychology. By GLENN D. HIGGINSON. New York: The Macmillan Co. 1936. Pp. xi+646. 12*s*. 6*d*. net.

The Psychology of Human Behaviour. By JOSEPH H. GRIFFITHS. London: George Allen and Unwin, Ltd. 1936. Pp. xxi+515. 12*s*. 6*d*. net.

Elements of Psychology. By KNIGHT DUNLAP. London: Henry Kimpton. 1936. Pp. 499.

Elementary General Psychology. By SAMUEL W. FERNBERGER. New York: F. S. Crofts and Co. 1936. Pp. 445.

In these days, when the study of motivations is very much to the forefront, perhaps some student will devote himself to a consideration of why very nearly every teacher of psychology seems sooner or later to succumb to the urge to write a general text-book. These five have all appeared in the course of the last six months or so. They differ in various respects, mostly rather minor ones: in the order of presentation of topics; in the emphasis accorded to points of view; in austerity or popularity of style. So far as content goes they are all much the same. Of necessity they report the same facts, they make use of much the same authorities, many of the illustrations included are derived from the same source.

Of these there is no bad book, and none which can be said to stand head and shoulders above its many companions. Knight Dunlap's, which is really a rewriting of his ***Elements of Scientific Psychology***, is perhaps the most clear-cut and the most original. At the same time it is a little less comprehensive than the others, and is written with less regard to the frailties of many students. Principal Banks has written mainly for students of education, and attempts to combine the Hormic and the Noegenetic standpoints. This is a South African book, and it seems a little sad that it should not make a more determined attempt to interest the student in the many psychological problems which are peculiarly pressing in that part of the great African scene. Dr Griffiths, Dr Higginson and Prof. Fernberger all write with authority and ease, and all of them with emphasis upon a functional point of view. Any one of these books can safely be recommended to an intelligent student, but whether there was a genuine need for them all is another matter altogether.

The Measurement of Interests. By DOUGLAS FRYER. London: George G. Harrap and Co. 1936. Pp. xxxvi+488. 10*s*. 6*d*. net.

Professor Fryer's book is already pretty well known in this country to students who are interested in modern developments of the psychological study of character and temperament. It is, however, of great advantage that this English edition should have been published, since it makes a most valuable and well-documented study readily accessible to English readers. It is, in fact, a complete survey of the methods of the questionnaire, the inventory, and assessment forms in general as they are used in the attempt to classify and to follow out the results of human interests. The book was published in 1931, and it seems a pity that some supplementary study has not been undertaken of developments in trait discovery and evaluation which have taken place since that date. Even apart from this, however, the book has a permanent value for its most careful and lucid description of methods and results. There is perhaps less critical evaluation than most English readers will desire; not quite enough effort to make clear the assumptions, both statistical and psychological, which the inventory, the rating-scale and the questionnaire demand. A more thorough consideration of clinical methods—not that these are entirely neglected—would add to the value of the book. But these are defects which can and no doubt will be remedied as new editions appear. In the meantime the volume presents a mass of material which the student and the teacher should carefully study, and from which they will have much to learn.

The Scientist in Action. A Scientific Study of his Methods. By WILLIAM H. GEORGE. London: Williams and Norgate. 1936. Pp. 355. 10*s*. 6*d*. net.

This does not profess to be a psychological treatise. It is a study, by a physicist who is profoundly interested in biological research, of the methods actually used by scientists in their search for explanations. The four main sections are called: The Scientific Outlook; Getting Scientific Facts; Arranging Scientific Facts; and Conclusion. A short, but very useful bibliography is added. Perhaps nothing that is very new emerges from the analysis, but the ways of the scientist at work are set forth with vigour, clarity and with plenty of illustration. Since there is a great deal about biological research in this book, it is not surprising that much stress is laid upon the notion of pattern: "Properties which cannot be seen as inherent in the properties of separate parts are called pattern properties, to express the idea of a relationship between parts and a whole." Facts are regarded as 'coincident observations' and the author leans towards a description or Patterning theory of science according to which "The various scientific arrangements of facts are regarded as patterning devices into which facts are fitted, and the part played by the human observer in each is explicitly stated." So brief an indication of the scope of the book is perhaps bound to give the impression that the author has contributed mainly to the philosophy of science. Yet he himself says, and rightly: "The point of view taken up in this book is throughout uncompromisingly non-philosophical." He is all the time concerned mainly with actual scientific researches, and he gets his principles directly out of these. The book is a valuable and concrete study of scientific methodology.

An Introduction to Psychological Medicine. By R. G. GORDON, N. G. HARRIS and J. R. REES. Oxford University Press; London: Humphrey Milford. 1936. Pp. x+386. 10*s.* 6*d.* net.

This book is intended to give the medical student "some picture of the structure and function of the normal mind, the nature of psycho-pathological processes, and the form taken by clinical entities which can be recognized and classified as the various forms of mental illness". It is thus not meant for the post-graduate student or mental specialist. It provides an extremely clear, straightforward, readable and comprehensible introduction to the subject of psycho-pathology, and should be of the greatest help to the student who intends to enter general practice. It shows him the importance of taking into account the psychological factor when diagnosing and treating physical illness. To this end it might have been better to have given an even fuller treatment of the aetiology and symptomatology of the psycho-neuroses in the light of modern psycho-analytical knowledge. The description of the psychoses had of necessity to be rather sketchy, and indeed details of medical treatment, apart from that necessary in an emergency, might well have been omitted. The first section of the book, on general psychology, is also so brief as to be of doubtful utility. But apart from these minor defects, the book should be of very real use to the student of psychological medicine.

Education and the Psychology of Thinking. By P. M. SYMONDS. London: McGraw-Hill Publishing Co., Ltd. 1936. Pp. xii+306. 15*s.* net.

The teacher and student of education will find in this book a clear, straightforward account of the more immediate and commonplace aspects of the processes which constitute thinking. These processes are first described and analysed separately under the headings of Formulation, Classification, Selection, Generalization, etc. The general process of syllogistic thinking is then analysed, and is followed by an excellent chapter on 'Imperfections in Thinking'; and the functions exercised by the above partial processes are indicated. Throughout, the discussion is illustrated by practical examples; and numerous applications to educational practice are described, which are expanded in a chapter on 'What Education Can Do to Improve Thinking.'

The psychologist must not expect to find a far-reaching theoretical analysis of thinking; indeed, the more difficult problems such as that of insight are scarcely touched upon. Nor will the moralist consider that the chapter on 'Thinking about Conduct' touches more than the fringe of the subject. It may, however, act as a warning to the simple-minded rationalizer, if only to make him more tolerant towards the shortcomings of others in that respect.

Zur Psychologie des Produktiven Denkens. Von KARL DUNCKER. Mit 27 Abbildungen. Berlin: Julius Springer. 1935. Pp. vii+135. RM. 9.60.

Dr Duncker has managed to include, in 135 pages, more interesting (and difficult) statements about thinking than one would have believed possible. The first part of the book, which treats of thought processes in simple technical and mathematical problems, is straightforward enough. The mental processes leading to the solution (or the failure to solve) certain problems upon which the author has conducted experiments are analysed in terms of content with great ingenuity. Particularly valuable are some observations upon Maier's concepts of 'Direction', 'Problem' and 'Elements to be combined'. In the second part, the argument passes to more philosophical regions. An attempt is made to analyse, and to define, psychological processes in terms of the logical relations subsisting within their content. There is a resolute attempt, for instance, to determine the nature of 'Insight'; a task hitherto neglected by the Gestalt school. In fact, a refreshing feature of the whole book is the author's evident dissatisfaction with the facile use of such notions as 'Prăgnanz', 'Closure' and 'Equilibrium' in the explanation of mental processes. But this particular attempt to solve such problems as that of the nature of insight by consideration of the logical relations of the psychological material involved, leads rather to the classical controversies of philosophy than to conclusions capable of translation into the simple language of experimental issue. As an intellectual exercise the book is remarkable, and must take its place in the notable tradition of the German 'Denkpsychologie'.

Infant Speech: A Study of the Beginnings of Language. By M. M. LEWIS. London: Kegan Paul, Trench, Trubner and Co. Ltd. 1936. Pp. xii + 335. 12*s.* 6*d.* net.

This book represents an admirably clear and detailed account of the development of speech in the young child, which is systematically traced out from records, such as those made by Preyer, Stern, etc., of the speech sounds and words used by individual children, and from the very detailed phonetic records made by the author from his own son. From these data the author makes a number of very interesting general conclusions as to the nature of infant speech development, and compares these with the specific conclusions of Meumann, Stern, Bühler and others: he also discusses, more briefly, general theories of language development such as those of Wundt, Jespersen, De Laguna, etc. Throughout, the author is concerned to show the importance both of innate and environmental factors in speech development. Especially interesting is his conclusion that the child's early cries, particularly the consonant sounds, are closely related to hunger states—the nasals to anticipatory sucking movements allied with the discomfort of urgent hunger, the labials and dentals to such movements without such urgent discomfort, and the gutturals to the gurgling sounds made during satiety. From such cries may be traced the development of the first meaningful words, such as 'mama'—the hunger cry associated with the mother. The traditional nursery usage of such words is thus developed from the child's spontaneous utterance, and in turn reinforces and stabilizes it by social approval. Expressive cries become in turn spoken words which are not at first mere labels for things, but affectively and conatively toned performances of acts dealing with the total situation. In the development of the objective reference of words, and finally of their conceptual use, functional similarities and differences between objective situations are of the utmost importance; but the individual and unconventional usage is finally subordinated to the conventional accepted use through the child's biological need for close contact with and manipulation of his social environment.

This book can be confidently recommended to the reader who wishes for a clear and systematic discussion of the experimental work and general theory relating to this fundamentally important subject, which has not, in the past, been given the attention which it deserves.

The Dangers of being Human. By EDWARD GLOVER. London: George Allen and Unwin, Ltd. 1936. Pp. 206. 5*s.* net.

This popularly written little book, with its introduction by Dean Inge, is based on talks recently broadcast by the author.

Dr Glover takes a typical psycho-analytical view of the dangers of being human which result from domination of the human being by irrational fears. One wonders at times if he is not poking fun at his audience and making statements merely for the joy of arousing controversy. He discusses the League of Nations, pacifists, education, the causes of war, politics and Utopias in a provocative manner. Communism, we are told, is doomed to ultimate failure because it "recognizes only one primary instinct, the instinct of self-preservation", and "does not pay as much attention to man's love needs and hate tensions". War is "a primitive attempt to control our own impulses which ends in our attacking the Old Adam in others". With one precept, however, most will agree: "If we can be brought to behave in a more humane and rational way to our own children we may be certain of increasing the happiness of future generations."

English Messiahs. By RONALD MATTHEWS. London: Methuen and Co. Ltd. 1936. Pp. xvi+230. 10*s.* 6*d.* net.

Most psychologists who essay the task will greatly enjoy reading this excellent study of six English religious pretenders. The characters dealt with are: James Nayler, Joanna Southcott, Richard Brothers, John Nichols Tom, Henry James Prince and the Rev. J. H. Smyth-Pigott. The stories of these people, of their hopes and fears, their successes and their failures are told in a swift but adequate and sympathetic manner. There is little moralizing and not too much psychologizing, and what there is of both is very good. Mr Matthews shows himself to be a critic of insight and kindliness, and his little book is both more interesting and more useful than many a more pretentious treatise.

Health, Sickness and Psychology: A Study of the Mind's Power for Health and Sickness. By R. W. WILDE. Oxford University Press; London: Humphrey Milford. 1936. Pp. viii+201.

Here is an attempt to put into simple and untechnical language some of the teaching and practice of modern psychotherapy. Part I describes some of the mechanisms by which mental processes can produce ill-health. Part II considers, largely by the help of case material, some common types of disturbance produced by mental process. Part III sets forth some of the principal forms of psychological treatment. The writing is clear and simple, and, probably by intention, not very critical.

Evaluative Research in Social Work. By ELEANOR T. GLUECK. New York School of Social Work: Columbia University Press; London: Humphrey Milford. 1936. Pp. iv+27. 1*s.* 6*d.* net.

In this address Dr Glueck describes what she calls 'evaluative research in the field of Criminology'. The general aim of the work is to find methods by which an accurate assessment may be made of the success of a given system of punishment or of treatment of delinquents. The study is based upon research carried out with about 500 former inmates of the Massachusetts Reformatory for men.

Psychology of the Vibrato in Voice and Instrument. By C. E. SEASHORE. University of Iowa Studies in the Psychology of Music, vol. III. Iowa: University Press. 1936. Pp. 159. $1.25 (paper cover), $1.50 (cloth bound).

Vol. I of the Iowa Studies, which contained a detailed account of experiments on the musical vibrato, was reviewed in this *Journal* in 1933 (XXIII, 408). The present volume is a less technical description of the same work, written for musicians and laymen rather than for psychologists and physicists. It contains numerous illustrations of records of the vibrato in songs, violin solos, etc., and it challenges the reader who may still be incredulous as to the universality of vibrato to carry out the following experiment. Take any vocal gramophone record of a well-known singer, play it at about half the proper speed; the ear will then easily be able to detect the pulsations in pitch and intensity which are always present but are seldom noticed in a normal performance. Perhaps the most interesting aspect of the work to the psychologist is the fact that the sensation of a vocal note which seems to be constant in pitch, intensity and timbre actually results from the fusion of a continuous vibrato in all three attributes.

Der Musikalische Schaffensprozess. By J. BAHLE. Leipzig: S. Hirzel. 1936. Pp. xv+253.

This is probably the most extensive psychological analysis of musical inspiration and composition yet published. The author surveys the theories of previous writers and culls as much material as possible from biographies of great composers. In addition he obtained the co-operation of some thirty living composers (including Strauss, Schönberg, Casella, Krenek, etc.), who, at his request, set certain songs to music and described for him their method of working. An English translation would be useful.

The Natural History of Mind. By A. D. RITCHIE. London: Longmans, Green and Co. 1936. Pp. viii+286. 15*s*. net.

This book, which is based upon the Tarner Lectures delivered at Trinity College, Cambridge, in 1935, consists of a study of the relation of the mind to its environment, in the light of recent physical and biological discoveries as to the nature of that environment. After discussing certain general theories of the nature of life and causality, the author enters in some detail into the physiology of the brain and nervous system, and then passes to an analytical discussion of certain problems of modern psychology. But though the author's outlook and method of analysis promise well, and though many interesting incidental points are brought out, the psychologist is left vaguely disappointed at the end of the discussion, because it seems to have done so little to further his understanding of those problems.

Thought and Real Existence. By G. DAWES HICKS. L. T. Hobhouse Memorial Trust Lectures, No. 6. Oxford University Press. 1936. Pp. 30. 2*s*. net.

Prof. Dawes Hicks gives a brief though extremely lucid account of the epistemological theory of the late L. T. Hobhouse, and indicates in a most interesting way the derivation of this theory from the attempt of Lotze to circumvent some of the more formidable difficulties in the Kantian Dualism. Though emphasizing the great advance made by Hobhouse in his rejection both of atomism and of the traditional opposition between sense experience and thought, Prof. Dawes Hicks is inclined to believe that Hobhouse allowed too much to current sensationalism, and recapitulates in this connection his well-known analysis of perceptual processes. The most important point wherein this analysis differs from that of Hobhouse is in an explicit definition of the content of any perceptual act as constituting an *awareness of the real object* upon which this act is directed. The significance of such a view for epistemological theory may well be considerable, but it can hardly be denied that all doctrines deriving from Brentano's Intentionalism contribute little towards a *psychological* elucidation of phenomenological problems. It is, however, necessary to remember that it is the Theory of Knowledge which at one time constituted the primary interest of Hobhouse, and which Prof. Dawes Hicks has made his special province.

The Mystery of the Mind's Desire. By JOHN FINLEY. The Kappa Delta Pi Lecture Series. New York: The Macmillan Co. 1936. Pp. viii + 48. 4*s*. 6*d*. net.

Towards a New Manner of Living. Swarthmore Lecture. By HOWARD E. COLLIER. London: George Allen and Unwin, Ltd. 1936. Pp. 99. 1*s*. 6*d*. net.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1936

Revenue Account for the year ended 30 September 1936

1935 £ *s. d.*	*1935*	EXPENDITURE	£ *s. d.*	£ *s. d.*
170 0 0		*To* Rent, 55 Russell Square		170 0 0
241 6 8		Secretary's Salary		252 0 0
3 9 4		*State Insurance*		
		Part-time assistance in Office		4 5 6
71 4 3		Printing and Duplicating		77 9 3
25 13 0		Hire of Halls		30 11 2
		Office and General Expenses:		
	£29 17 9	Caretaker, Lighting and Heating	27 6 11	
	14 12 0	Stationery	15 14 7	
	64 2 1	Postages and Telephone	74 4 4	
	4 12 0	Fire &c. Insurance	4 12 0	
	4 14 6	Subscriptions to Outside Associations	4 14 6	
	10 10 0	Audit Fee 1935 and Honorarium	15 15 0	
	17 9	Repairs	1 14 4	
	15 15 8	Teas	16 10 8	
	6 14 1	Sundry Expenses	4 17 5	
151 15 10				165 9 9
		Journals Publication Expenses:		
	195 4 10	General	239 3 6	
	134 6 10	Medical	141 4 4	
	85 2 6	Educational	98 0 0	
414 14 2				478 7 10
1 11 0		Work for Committee on Human Mental Measurements		6 9 1
5 1 5		Midland Branch Expenses		4 12 1
7 5 11		*Northern Branch Expenses*		
11 1 8		Scottish Branch Expenses		10 13 5
		Revision of Members' Register		22 3 9
1 0 2		Cheque Books and Bank Charges		1 10 0
18 18 0		*Diplomas of Honorary Membership*		
		Publications Reserve:		
		Amount transferred		50 0 0
		Depreciation:		
		Library	66 4 4	
		Office Furniture and Fittings	21 18 0	
85 16 9				88 2 4
1208 18 2				1361 14 2
20 19 0		Balance, being Surplus of Income over Expenditure		57 6 7
£1229 17 2				£1419 0 9

1935 £ *s. d.*	INCOME	£ *s. d.*	£ *s. d.*
	By Subscriptions 1936	1324 2 6	
	Subscriptions 1934 and 1935	7 7 0	
		1331 9 6	
	Less Resignations and Removals	100 1 6	
1114 2 6			1231 8 0
115 14 8	Interest on Investments		117 9 6
	Recovery of Income Tax		70 3 3
£1229 17 2			£1419 0 9

Balance Sheet as at 30 September 1936

1935 £ s. d.	LIABILITIES	£ s. d.	£ s. d.
4 19 6	Subscriptions paid in Advance		2 7 0
	Publications Reserve:		
72 0 0	Balance as per last Balance Sheet ...	72 0 0	
	Add Amount transferred from Revenue Account	50 0 0	122 0 0
	Revenue Account:		
	Balance as per last Balance Sheet ...	4553 13 4	
4553 13 4	*Add* Surplus of year	57 6 7	4610 19 11
£4630 12 10			£4735 6 11

1935 £ s. d.	ASSETS	£ s. d.	£ s. d.
72 8 5	Cash at Barclays Bank Ltd.		162 9 0
2 2	Postage stamps in hand		3 5
	Investments at Cost:		
	£300 5 % Conversion Loan 1944–64	318 9 9	
	£3026. 19*s.* 1*d.* 3½ % Inscribed War Loan	3125 7 9	
3443 17 6			3443 17 6
	Note. Market value of above Securities at 30 Sept. 1936, £3623.		
	Sundry Debtors		9 2
117 18 6	Subscriptions in Arrear		126 10 6
	Office Furniture and Fittings:		
	Balance as per last Balance Sheet ...	207 1 0	
	Add Expenditure during year ...	12 7 0	
		219 8 0	
	Less Depreciation	21 18 0	
207 1 0			197 10 0
	Library:		
	Balance as per last Balance Sheet ...	565 10 0	
	Add Expenditure during year ...	96 14 0	
		662 4 4	
	Less Depreciation	66 4 4	
565 10 0			596 0 0
	Monograph Publications:		
	Balance as per last Balance Sheet ...	173 15 3	
	Less Amounts repaid during year ...	15 7 11	
173 15 3			158 7 4
50 0 0	Shares in the British Journal of Educational Psychology, Ltd., at Cost ...		50 0 0
£4630 12 10			£4735 6 11

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Bankers' Certificate.

T. BARRETT,
Incorporated Accountant

63 EASTCOTE LANE.
SOUTH HARROW.
27 *October* 1936.

S. J. F. PHILPOTT,
Hon. Treasurer.

VOLUME XXVII APRIL, 1937 PART 4

METHODS AND ASSUMPTIONS OF FIELD WORK IN SOCIAL PSYCHOLOGY[1]

BY O. A. OESER

(*From the Psychological Laboratory, St Andrews*)

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Part I. *The methods and assumptions of field work* (pp. 345–355).
  (1) *Introduction* (pp. 345–346).
    (*a*) *The study of behaviour* (p. 345).
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  (2) *Definitions* (pp. 346–347).
  (3) *Anthropology at home* (pp. 347–348).
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  (6) *Procedures and methods* (pp. 350–355).
    (*a*) *The team* (p. 351).
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Part II. *Preliminary indications and further problems* (pp. 355–362).
  (1) *Social psychology and vocational guidance* (pp. 355–360).
    (*a*) *The restriction of vocational wishes* (p. 355).
    (*b*) *Terminology of vocational psychology* (pp. 355–356).
    (*c*) *The analysis of social types* (pp. 356–360).
  (2) *Conclusion* (pp. 361–362).
Summary (p. 362).
References (p. 363).

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[1] This paper was read to the Psychology Section of the British Association for the Advancement of Science, Blackpool, 1936.

It deals with fundamental aims and methods in a series of *Co-ordinated Social Researches in a Scottish Area* which are being carried out under the general direction of the author, Dr O. A. Oeser. It is considered desirable to publish this contribution forthwith in the hope that it may stimulate discussion and research, since the methods and ideas put forward appear to possess a very wide application. In the complete report, which will be issued in due course, the present paper will be supplemented by others, written by colleagues of Dr Oeser, dealing in a more detailed way with important aspects of the methods which are herein indicated and presenting the factual details of the particular survey which Dr Oeser and his associates are now carrying out. (EDITOR.)

PREFACE

IN July 1935 the Trustees of the Pilgrim Trust agreed to finance for three years a research project on unemployment, more particularly juvenile unemployment. The work is being carried out by members of the staff and research staff of the Department of Psychology at the University of St Andrews, and was begun in October 1935.

The basic assumptions underlying our general approach are three:

(i) That for some time social psychology, if it is to become a science at all, must turn away from philosophy and from the systematic textbook, and must apply itself whole-heartedly to *field work*. This process has already begun and numerous papers in psychological journals on the results of diverse questionnaires and surveys show how strong the movement is becoming.

(ii) That the *methods* (not theories) of the other social sciences, such as cultural anthropology, sociology, and economics, are fully relevant to field work in social psychology. Therefore, a constructive synthesis of these methods must be attempted and carried into practice.

(iii) That such a practical synthesis is beyond the powers of one man or one department and the solution, if any, can be found only in *team work* by various specialists.

This does not mean that a horde of inquisitive scientists should be loosed on a defenceless community. But it does mean that a small group of five or six specialists in the social sciences (and in these we include medical psychology) shall study at first hand some specific problems in a real society. They must apply their special interests, attitudes and methods simultaneously to the same problems; and, by close association in the field and around the conference table, hammer out a common theory of method and a common terminology, whilst refining the specialized techniques in their own subjects.

These are practical demands and proposals. They seem 'ideal' only because the progress of investigation is ponderous and the process of agreement slow; agreement and the progress of investigation being very dependent on the qualities each member contributes to a research group. In the course of time, field *techniques* will be substituted for more and more of these individual qualities.

For example, to penetrate deeply into the central attitudes of strangers requires a high degree of tact on the part of the investigator. At present, tact is a personality characteristic; but the procedure can be standardized and taught up to a certain point, as a definite field technique. Naturally, not everyone is capable of learning this technique thoroughly. But the same is true of physics; not everyone can learn to handle apparatus competently enough to tackle research. The methods of functional penetration and *indirect* questionnaires, which will be discussed later, are methods of transcending the personality characteristics of any one investigator and applying them as field techniques.

This paper is designed to enlarge on some of the above aspects and also to serve as an introduction to the more specialized, technical and factual papers of my colleagues.

PART I. THE METHODS AND ASSUMPTIONS OF FIELD WORK

(1) *Introduction*

To introduce this paper, let me state quite briefly and categorically what are the important methodological borrowings for field work in social psychology.

From the point of view of the empirical social psychologist, the *methods* applicable to his own work are drawn from two groups of sciences:

(*a*) *The study of behaviour.*

(i) The methods of *observation of behaviour* in psychology have been most carefully applied and refined in the study of young children. I need merely refer to the work done at Yale and Vienna.

(ii) The methods of *documentation and statistical analysis* of most obvious use are those developed to a high level of perfection in the study of personality, temperament, traits, and types.

(iii) Psychiatric and psychoanalytic procedure provides methods of observing and interpreting *abnormal responses to social situations.*

(*b*) *The study of the social settings of behaviour.*

(i) Anthropology has developed methods for studying the structure and content of *societies everywhere.* It is now teaching the relativity of human institutions and the extreme plasticity of the human being, and is further undermining the already moribund conception that instincts are the basic phenomena in social psychology. Above all, anthropologists have developed some useful methods of documentation and observation, and that non-valuing attitude towards cultural phenomena which is necessary for the purposes of a science.

(ii) Economics, sociology and related sciences have developed methods for analysing some of the important institutions and their paraphenomena in the *society around us*. Economics is particularly important. The urge to amass wealth and to seek opportunities for display are two closely connected intentions in western European culture, and economics has developed methods for dealing with the paraphenomena of these drives. Further, the historical approach to specific problems in economics and sociology is common to both sciences.

(2) *Definitions*

It is necessary at this point to introduce some definitions of words that throughout these papers will be used in a special sense:

(*a*) One of the basic (ultimate) phenomena of social psychology is that of *the reification of concepts*. This means that we have—as an observable fact—a strong tendency (propensity) to assume that the symbols (words) expressing the relations between certain concepts represent things as 'real' as those represented by the symbols (words) for the things or concepts between which the relations have been established in thinking. More briefly, when we have a word for a certain system of relations, we imagine that this word describes something having as substantial an existence as the things between which the relations hold. Thus, 'group' is a name for certain observable collections of people under certain observable conditions. When we next speak of 'the group mind', believing it to possess the same kind and degree of reality as an individual 'mind', expecting 'it', 'the group', to behave like an individual with certain peculiar temperamental properties, we are guilty of the fallacy of reification.

(*b*) The fact that we can be proven guilty of a logical and epistemological fallacy does not, however, prevent us from acting on the assumption that there is a group mind, for example. The arguments in McDougall's book with this title are proof that even academic psychologists do so. Current politics prove that people assume and act upon such reified concepts as 'the State', 'the Law', 'the Nation', all conceived as supra-individual entities. F. H. Allport (3) gives numerous examples of this process. He is, however, chiefly concerned with demonstrating the false assumptions underlying such reifications.

The point, therefore, which must be kept clearly before us is that empirical observation presents us with the *fact* that concepts are reified, and that this fact determines behaviour as concretely as an ideal or sentiment or any other complex stimulus determines it. *Behaviour determined by such reified concepts is an example of what will be called paraphenomenal behaviour.* For one of the paraphenomena of institutions is the reified concept of that institution. This applies to all institutions irrespective of size: the family, the school, the club, the state.

(*c*) *Paraphenomena*[1] (of institutions) may now be defined as those stimuli to institutional behaviour which are consequences of an institution existing as a *system*, not a collection, of relations between individuals. If we regard paraphenomena in this way, we see that considered as stimuli they can be studied in and for themselves. The 'responses' of social psychology are obviously not brought about by the same kind of 'stimuli' as are the responses dealt with by behaviourism proper. When we attempt to define stimuli for the psychology of institutional behaviour in the same way as we do for the psychology of perceiving, namely in terms of the physical sciences, complete confusion results. The 'stimuli' are of a different order and need a different level of analysis before they can be defined. Therefore we propose to speak of paraphenomena and paraphenomenal behaviour. A reified concept of an institution is a paraphenomenon of that institution and gives rise to certain forms of behaviour. These definitions are operational just as much as, say, the definition of 'fatigue' in physiology (cf. E. D. Adrian (1)).

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[1] Pres. part. pass. used substantivally of παραφαίνω: 'show alongside', pass. 'appear alongside'. The word *epiphenomena* is to be avoided because of its philosophic connotation and an implication of unreality.

(*d*) We may put the meaning of *paraphenomena* in another way: whenever an institution has been established, further social elaborations result.

The nature and laws of these elaborations can often be studied in and for themselves. Thus economics studies the "laws of the trade cycle", i.e. the effects flowing from the economic institutionalization of our culture.

The history of some institutions, e.g. the Church, the Labour Party, shows clearly how the paraphenomenal behaviour gradually clogs the smooth working of the institution, so that it becomes unworkable. To some extent this is true of 'bureaucracy' also (cf. Lord Hewart of Bury (11)).

(*e*) *Ritual* is behaviour designed to impress the onlookers as well as the members of an institution with the existence or strength or purpose of an institution. Ritual is used largely in two ways:

(i) for display,

(ii) for setting up as nearly as possible the same attitude or attitudes in the members. In the sense that all members have closely similar attitudes towards the expressed aims of an institution, we may speak of ritual as being a *directive for collective action.*

Paraphenomenal behaviour is behaviour *not* deliberately designed (i.e. not determined by the rules of ritual) but still occurring of necessity because the agent is a member of an institution (cf. p. 357, footnote on *autogonous laws of institutions*).

Both ritual and paraphenomenal behaviour may be subsumed under F. H. Allport's wider term *institutional behaviour.*

(3) *Anthropology at home*

We have to beware of minimizing the complexity of our social structures. Nevertheless, there is one error that has in the past tended to confine the activities of cultural anthropologists to minority groups or to 'primitive' tribes outside Europe. This is the error of thinking that 'primitive' societies are *simple* societies, and should therefore be studied first; and this error is a relic of psychological atomism and an unsophisticated evolution theory. A primitive society is now usually defined as one that has no written records. Anthropologists like Radin and Sapir definitely take the view that 'simple' and 'primitive' are not synonymous. Indeed, if we consider how much richer the languages with written records are, it seems plain that the description in words of the attitudes of, say, the Zulus, is likely to be a more difficult task than it is in our society. This does not mean that we should abandon the analysis of primitive cultures; it is merely an argument for prosecuting the analysis of our own, now, with similar methods and the same outlook.

(4) *The problem*

These and a number of related considerations led us to assume that social psychology, to be successful, needs the co-ordination of the methods of the other sciences of man, and the co-operation of a team of workers trained in different ways. It was decided to test these two assumptions in practice, and to attempt a problem that is of interest to the theoretical psychologist and is also of public importance: Unemployment.

For psychological theory, the study of unemployment should yield some interesting material. In unemployment, one of the fundamental axes of a man's social space or frame of reference is missing. Put in another way, one of the basic intentions behind observable behaviour in Western society, 'the will to work', is deprived of an object. This is a major operation and one that can be undertaken only in a real society. In a laboratory it is not possible, as yet, to deprive a man of one of his centres of integration, to remove one dimension from his social space. We can do that sort of thing only for the space of sense perception.

Put in this way, it is at once obvious how important a subject for the psychologist unemployment is. By studying juveniles who are unemployed for periods of various durations, we may hope to find the laws according to which progressive breakdown occurs or progressive reorientation in another frame of reference is established.

'*The will to work*' is a colloquial expression, the meaning of which may be analysed as follows: Every society, every institution, is self-preserving, in the sense that as a society or institution it continues, though its composition in terms of individuals is continually changing. In the individual member of *our* society, this time-independent aspect is reflected as a constant drive to maintain his place in it by earning *wages*, i.e. as 'the will to work'. These wages—in the broadest sense of the word—give him both a static place in the society and the dynamic possibility of achieving a 'higher' place. When an individual becomes unemployed, he loses both his place and the possibility of 'getting on'.

Common sense indicates, and (what is beyond the scope of this paper) a consideration of economic history since the beginning of the nineteenth century very specifically proves and underlines, that we must speak of the *need to work*, certainly in the case of industrial workers in a city, who can subsist only through earning wages. We have not the right or the evidence to call this the *will to work*. The latter phrase is a slogan. Only those can understand a slogan who are thoroughly familiar not only with the language in which it is expressed, but also with certain details of the social history and present state of the society in which it is current.

The slogan of 'the will to work' is of interest to psychologists, because in order to know how it has come to be formulated and accepted we have to keep in mind at least four things: A now discarded faculty psychology; that this arose out of a philosophy strongly tinged with theological dogmas; the premium put on the propensities of acquisition and display in our culture; and the economic history and structure of our society. These four influences caused us to accept 'the will' as a moral 'faculty', so that 'the will to work' and economic acquisitiveness were made into 'virtues' (i.e. attributes giving prestige). All these aspects go to form the rather muddled connotation of this slogan. I have heard an economist maintain that unemployment weakens 'the will to work', and a psychologist proposing to measure its strength. When we look at what an early Royal Commission called "The State of the Labouring Poor", we see how nonsensical, as regards 'will', such statements and proposals are.

(5) *The approach: related problems*

As soon as we begin to think a little around the problem of unemployment, we see that it is intimately related to a series of other problems. So intimate is this relation that we cannot study the problem itself *in vacuo*. We cannot, for instance, speak of the structure of the unemployed frame of reference without comparing it with that of the society of which the unemployed are geographically and administratively a part and with which they have a number of important interests and attitudes in common. We must begin, therefore, by seeing the juvenile unemployed in their social settings: the home, the school, the street, the city. Each of these settings has a wider setting; and economics, history, sociology, must each contribute a share to the analysis of these wider settings. The further away we get from the individual, the more abstract, i.e. difficult to formulate, these settings become, and the more liable we are to confuse our abstract concepts with living realities.

What we want, therefore, is a system of reference which will not confuse us linguistically. We cannot take over the concepts or even the words used by economists and sociologists. What we can and must take over are principles of method, and in addition, if we can justify their use in practice, actual techniques of field investigation and documentation. If we think in terms of method, we can proceed to systematic exploration of the forms of social behaviour and the structure of social institutions. Only in this way shall we be able to arrive at inductive generalizations of psychological laws governing the attitudes and behaviour of a social group.

This is not merely an ideal demand. It is already possible to show that a law can be formulated which governs the number of roles which must be assumed by observers in order to penetrate a field of attitudes completely. This law will be stated and demonstrated by one of my colleagues in a later report.[1]

The first step, then, in approaching a problem of this kind in social psychology is to give as adequate a description as possible of the total cultural setting. "If we are interested in cultural processes, the only way in which we can know the significance of the selected detail of behaviour is against the background of the motives and emotions and values that are institutionalized in that culture" ((4), p. 49). That is an important hint from an anthropologist. This quotation from Ruth Benedict well summarizes a trend in present-day anthropological thought. Similar passages could be quoted from Malinowski, Radcliffe-Brown, Mead and Sapir. It is generally agreed that modern field anthropology has demonstrated that meaning can be assigned to cultural elements only in relation to their function—however one analyses 'function'—in the culture as a whole. What is true of, say, Melanesia, is also true of Scotland: before we attempt the detailed analysis of any one aspect of a culture, we need to know the institutions and then the motives and values that have been institutionalized in that culture.

In our work on the psychology of juvenile unemployment we hope to adapt Thurstone's attitude scales (26). Here we have a valuable method—an extension of the psycho-physical methods of experimental psychology—which can be effectively used in social psychology. But before we can draw up a questionnaire by means of which attitudes can be measured along Thurstone's psycho-physical scale, we must know the field. In order to know the attitudes found in a field, we have to make use of more indirect methods of penetrating to the dominant intentions operating in a cultural field. That is, we have to construct a questionnaire which will be applied indirectly, in the form of conversation.[2]

(6) *Procedures and methods*

What follows gives a brief survey of the procedures which are being employed in approaching the study of unemployment. A detailed theoretical analysis of the methods to be used in general in empirical social research will be published elsewhere.

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[1] Cf. *functional penetration*, p. 352.

[2] Cf. G. W. Allport's criticism of Thurstone's scale ((2), p. 831). Allport sees the difficulty of applying Thurstone's scale, but does not see the way out, which is by means of the indirect questionnaire method.

(*a*) *The team.* The first step is, naturally, the choice of the team. It must consist of workers trained in different aspects of the social sciences, with sufficient common training in psychology to enable them to wield the same terminology in discussion. In addition they must be able to co-operate fully with each other in a temperamental sense. Our observers are:

(i) Four psychologists, each a specialist in a different field. One is a social psychologist and ethnologist, one an educationist, one an industrial psychologist, and one has had experience of American psychological survey methods.

(ii) An economist who is also a historian, having an acquaintance with sociological theory and practice.

(iii) Next year we hope to add a psychiatrist with wide experience of child guidance clinics, and one or two experts on nutrition and family budgets.[1]

None of the workers is a native of Scotland. They come from England, Ireland, the U.S.A., Germany and South Africa. Thus one of the difficulties of social research is to some extent met, namely, the difficulty that it is not easy for the native to view the behaviour of his cultural group objectively.[2]

(*b*) *Training observers.* The second step is the training of the observers in the actual field. This is a process that has no end, for experience is accumulating all the time. It has proceeded intensively in three ways:

(i) In weekly seminars by constant, exhaustive discussion of methods, results, and relevant literature.

(ii) By having to make the necessary contacts with administrative authorities in the city, education and health services, the executives of industry and the Ministry of Labour, social workers and unemployed clubs; and by visiting and observing the various activities in which juveniles are employed in industry and commerce.[3]

(iii) The observers have been subjected to a number of psychological tests, so that we may know as much about them as we hope to know about some of our subjects later on. If one knows the temperamental characteristics and psychological type of the observer, it may be possible to guard against over-emphasis and value judgements about similar types in the culture or about institutions that favour a certain type.[1] This is somewhat conjectural at present. In one or two years' time it may be possible to speak more authoritatively about the value of this precaution. There is, however, no doubt that a knowledge of the observer's dominant attitudes and beliefs is essential, in order that one may discount bias in selection and observation. I need not labour this point, since it is very fully—and, for some reputations, devastatingly—dealt with by Paul Radin ((24), pp. 64ff. and particularly Chap. IV).

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[1] This is not meant to imply that the composition of the team is ideal. It would, for instance, be useful to have a fully trained cultural anthropologist.

[2] The functions of, and limits to, the 'stranger value' of observers in field work will be fully analysed by one of my colleagues in a later report.

[3] This seems the best place for publicly recording our thanks to the numerous individuals behind the institutions mentioned above, for their unfailing patience, courtesy, and help. Without their generous help we should not be able to penetrate very far into a complex industrial city.

(*c*) *Functional penetration.* The third step, which in practice is contemporaneous with the preceding—the training of the observers—is the functional penetration of the field. The work of the Viennese Institute of Psychology has shown how important it is (14). Neglect of this fundamental method has effectively limited most 'Surveys', as is at once obvious if we compare *Marienthal* (14) with *Middletown* (17). The former gives a considerably deeper psychological insight into the community.

The method of functional penetration is so important that special attention will be devoted to it in a later report. I need therefore say only what we mean by the term. We mean that the observers approach the community to be studied not as reporters with notebook and camera, but as far as possible as accepted members of that community, having several definite and easily intelligible functions within it. It is clear that the frame of mind with which questions will be answered by a member of a community will depend on his attitude of acceptance, rejection, or neutrality towards the questioner. On the other hand, the fact that an observer has a part to play in a community makes it not only easier for him to ask questions, but will suggest many observations and questions that might otherwise not have occurred to him.

Perhaps the most important aspect, however, is this: interviewing and reporting have been systematized by means of the questionnaire. Now it appears that in the U.S.A. people have got used to having questionnaires sent by post or brought to the door by sociologists. They have become 'questionnaire-conscious'. In Great Britain, particularly in Scotland, this approach is not so well received. Moreover, there is no guarantee that the questions will not be answered in a spirit of levity, or even simply misunderstood. Even in the U.S.A. questionnaires are used mainly for the study of attitudes towards institutions, attitudes, that is, which are more or less peripheral and can be readily formulated. But for deeper, more central personal attitudes this method is quite inadequate. They must be studied by the more indirect methods such as a clinical investigator might use. In the society we are studying we cannot even go around to doors merely to ask questions. The indirect method, therefore, becomes of supreme importance to us. As a method, it involves applying the clinical approach systematically. That is, the questions to be asked are known to the observers, are introduced into conversations as they arise, and the answers are noted as soon as it is physically possible. The effectiveness of such a questionnaire (of all questionnaires for that matter) depends on whether it has been constructed with knowledge of the dominant interests and attitudes of the group. *This knowledge is acquired in the process of functional penetration of the field.*

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[1] With regard to the point that one ought to know the psychological *type* of the observer and of dominant groups in the culture being studied, it should be said here that such a procedure has been for many years explicitly advocated by E. R. Jaensch (cf. (13), pp. 27ff., 60 ff.; (12), p. 23 footnote, etc.). The question is discussed more fully in O. A. Oeser (23). It is mentioned sporadically in the works of typologists like Kretschmer and Jung. Anthropologists are now taking this question into account. Thus, R. Benedict (4) makes a Jungian attempt to distinguish Apollonian and Dionysian cultures, and E. J. Lindgren (15) points out that an investigator may be of the wrong psychological type for some cultures or subcultures, and mentions Kretschmer's types.

(*d*) *The social roles of the observers.* (i) The first approach of the team is as a group of inquiring scientists, *but with an immediately practical aim.* That is, we want to understand in order to help.

This is not a Machiavellian subterfuge, but a formulation of two elementary historical facts,

(α) that any theoretical science based on inductions from empirical data is capable of application;

(β) that the type of problem studied *intensively* in any period is closely connected with the needs of that period.

The view held by some that 'pure science' operates independently of its social setting will not bear historical scrutiny and is another aspect of the pervasive propensity to reify concepts, and to talk of 'the spirit of science' as though it were a theomorphic entity. Moreover, there is no need for the social scientist to hold himself aloof all the time. He is the only one who can adequately interpret his activities and findings.

If, then, we formulate these historical facts as above—that we come as inquirers with a double aim: to increase our knowledge, and to give what help we can—we make our efforts intelligible, are accepted by the community, and achieve the first step of gaining access to our material.

(ii) In order to get to grips with the practical aspects of juvenile employment and unemployment, we propose to test about 1000 school leavers, aged 14, for intellectual and other abilities and aptitudes. Here we are additionally fortunate in having access to the records of the International Examinations Enquiry being carried out in Dundee, as well as to the test results from Dundee schools, collected for the Mental Survey of Scotland in 1932.

The testing, then, has four aims:

(α) to aid in the functional penetration of the field;

(β) to discover the actual distribution of various measurable abilities;

(γ) to find out what effects prolonged unemployment has on these abilities;

(δ) to serve as a basis for vocational guidance on a broad scale by eliminating waste on the one hand, and aiding in the adaptation of the juvenile to industry on the other (cf. (27)).

For the larger scale vocational guidance carried out by Juvenile Advisory Councils and the Juvenile Employment Bureaux, it is essential to have detailed knowledge of the past and present economic and sociological structure of the area as well as an indication of future economic trends. It is at this point that the work of the economist in the team becomes very important.

Furthermore, for vocational guidance on such a scale as is in fact practised by the Ministry of Labour, knowledge of the detailed social stratification of the area is essential. Some aspects of this are discussed below.

Finally, the analysis of vocational wishes, preferences and interests, and of industrial attitudes is necessary for vocational guidance and of great interest to social psychology proper. These points will be illustrated and elaborated in the full report (see footnote, p. 343).

(iii) Following up the careers of these juveniles—important in itself for the theory and practice of vocational guidance—leads naturally to the examination of housing conditions, family life, and working conditions; whilst the test situation itself and the following up provide data for the analysis of the juvenile's wants, interests, vocational wishes, and social symbolizations.

The medical consultations are of practical use to the persons examined and therefore an important method of functional penetration. They are the most efficient setting for the study of breakdown phenomena among the social attitudes, as well as for the more recondite psychoanalytic theories of society.

There remain some humbler ways in which the observers assume intelligible roles: making collections of stamps and clothes, rent collecting for social settlements, giving lectures in unemployed clubs, and so on.

By these activities many contacts are facilitated and, more important, openings and diverse topics for conversation are at once to hand.

PART II. PRELIMINARY INDICATIONS AND FURTHER PROBLEMS

This paper serves partly as an exposition of method, and partly as a general introduction to the more technical and factual contributions of my colleagues. In what follows, therefore, I wish to do no more than indicate some of the directions in social psychology proper in which we have found problems and the general nature of some of these problems.

Let us follow out the implications of one factual analysis.

(1) *Social psychology and vocational guidance*

(*a*) *The restriction of vocational wishes.* The form used by the Ministry of Labour for registering at the Juvenile Employment Bureau (J.E.B.) the scholastic attainments and industrial future of the boys and girls who leave school at 14 contains the question: "What kind of work would you most like to have?" When the answers to this question at the April school-leaving period were classified into employment categories, it was found that ten categories and one for 'undecided' covered the vocational preferences of the girls, but more than forty-six categories were needed for the boys. This is a tremendous difference, made all the more striking when we examine the actual categories more closely.

We know that in our society the male is dominant and that the entry of females into various types of work and professions has been, and still is, everywhere accompanied by the expression of violent prejudices about the natural functions of women and by more active opposition—as in the recent decree excluding women from the diplomatic service. But it is interesting, and important, to see the prejudices operating so strongly at the age of 14 that girls have in fact a highly restricted sphere of wishes.

(*b*) *Terminology of vocational psychology.* It is necessary at this point to bear in mind the important distinctions in attitude between vocational phantasies, vocational wishes, vocational interests, vocational preferences, and vocational expectations. In the literature on vocational guidance these distinctions are ignored or at best blurred. It is when we see the problem of vocational guidance in its reference to the psychological and sociological structure of the child's environment that the distinctions come to be important, in fact fundamental to any vocational guidance policy.

For our present purpose we need note only two additional points. First, there is a scale of increasing social reference in the five nouns qualified above by 'vocational'. Phantasies are most closely determined by *individual* experiences; expectations by the conditions of the *labour market* in the area. Wishes, interests and preferences are determined by a number of factors, including individual abilities and education on the one hand, and on the other by the occurrence of certain occupations in the area.

Secondly, the children's answers to the Ministry of Labour's question are very largely an expression of vocational *expectations*. This is clear from our field notes of the vocational interviews conducted by the J.E.B. But wishes and interests also play a part, as these are determined to some extent by what the working class child actually knows about its working and its human environment. This being so, the argument above, which is couched in terms of vocational wishes and preferences, remains unaffected by considerations of the detailed analysis of vocational terminology.

With a little ingenuity, suitable operational definition[1] and more detailed investigation, it should be possible to give a quantitative measure for the degree of restriction of vocational wishes, interests, etc. An attempt to do this will be made. Meanwhile, we may note that *this restriction must be governed by a powerful symbol or fiction as to the relative position and power privileges of men and women in the society we are studying*.

(*c*) *The analysis of social types*. We may next link up the fact of vocational restriction with some facts obtained by observation alone. There are many situations in the city in which the roles of men and women are reversed in a fundamental respect: the women are the breadwinners and the men are unemployed. A special term is used to describe these men. They are called 'kettle-minders'. There is, therefore, a hiatus between occupational attitudes and occupational reality in hundreds of families. In terms of Gestalt psychology—which draws so many of its analogies from physics—there is a system of forces in tension; or in terms of psychoanalysis, there is a conflict between individual intentions and reality. The approach by way of an electrical or dynamic analogy may lead us to devise some way of quantitatively expressing the 'tension'—if this word is suitably defined in psychological terms (more accurately, in terms of *behaviour*). The approach by way of psychoanalytic theory leads us to pose further questions.

If there is a conflict between the intentions of the individual and reality, we must expect to find evidence of repression, dissociation, rationalization, sublimation, and, in extreme cases, of psychoneurotic breakdown. Each of the methods adopted by individuals for dealing with this conflict will modify some existing attitudes or create new ones. If the same attitudes are adopted by a large number of individuals, we shall expect to find new social symbols arising, which may even be institutionalized. If we pursue this line of investigation still further, we may arrive at the underlying psychological laws governing the rise of *one* institution and its subsequent reification. Once the reified institution has become a reality to individuals, it acts as a 'force', in the sense that it determines behaviour by virtue of its autogonous laws.[1]

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[1] *Operational definitions* in psychology are discussed in the author's "Critical notice: Koffka's *Principles of Gestalt Psychology*" (22), pp. 101f.), and in Boring's "Special review" of the same work (5). Cf. also p. 347 *supra*.

The next question to be asked is as to the historical antecedents of this particular pseudo-institution to which the name 'kettle-minding' has been given. The economic history and analysis demonstrate that we are here dealing with a case in which long-period unemployment in Dundee is not relatively accidental or sporadic, but is a necessary consequence of the history and structure of the jute industry in the world as a whole. After 1870, in order to meet the growing competition, female and juvenile labour was employed to a steadily increasing extent. During the present century, rationalization was speeded up, especially in the last ten years. This rationalization means not only that fewer workers are employed per machine unit (e.g. spindle), but that younger and less skilled labour can be employed, at lower wages.

A further significant fact is that factory welfare-workers, foremen, and even government officials, believe that women are better weavers than men, *and that this skill is hereditary in girls.* Foremen, who usually enrol new labour, speak of 'weaving families'. At a vocational interview, one of the committee was shocked when a girl said she wanted to be a carpet weaver. Carpet weaving is done by men, not women, although in fact it is said to be not as difficult as jute cloth weaving, which is done by women. We see thus how in a short time (since about 1870) the employers' procedure of enrolling women as weavers has been rationalized in the community as a social fiction: "Women are better weavers than men."[2]

Adult male unemployment in this particular city, then, is a paraphenomenon of the economic structure of the jute industry. It is necessary, not seasonal. It has become a tradition. Our studies of the attitudes of 'kettle-minders', therefore, will show how this tradition of male unemployment in an otherwise male-dominant culture has been dealt with, what adjustments have been made by individuals, and how it has become institutionalized.[1]

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[1] *autogonous* (*αὐτο* + rt. *γον*), 'naturally or of itself produced or born, self-produced' (Liddell and Scott). This word is used to indicate the following: Whenever an institution (e.g. a club) has been formed, the structure of this institution, as laid down in written form or embodied in a certain number of traditions, will form a unity. This unity will give rise to other laws—in the sense of stimuli for, or inhibitors of, certain kinds of behaviour—not foreseen in the constitution or structure. That is, the additional laws, though they may perhaps be apparent to a close analytical and psychological scrutiny of the implications of the constitution, are accepted by members of the institution as being 'natural', 'obvious', or 'instinctive'. In this sense, every institution has *autogonous laws.* (The words 'autonomous' and 'autochthonous' are each to some extent synonymous but are to be avoided because of their already well-established connotations.) These autogonous laws regulate the paraphenomena of institutions. In this connexion it is instructive to read A. V. Dicey (7).

[2] With regard to the low level of wages accepted by women, vide P. Sargant Florence ((9), pp. 20–37).

The above analysis shows clearly the advantage of field work in a concrete case. Instead of talking of 'unemployment' in the abstract, we have resolved the term, in one case at least, into a study of types of unemployment and a study of cultural, economic and historical backgrounds. We may now, in a real sense, speak of a *social type*.

In Dundee, as elsewhere, there are many different social types. Among them is the one created by the traumatic effect of unemployment on a man who has been employed in an occupation having a high prestige value. This is the type of unemployment discussed by Lazarsfeld in his *Marienthal* studies (14). He gave the impression that a traumatic effect is always the result of unemployment; but he was dealing with a highly special community, which grew up around a single factory and became unemployed almost over-night when this factory shut down. In our study of the 'kettle-minders' we have not found the type of effect described so well by Lazarsfeld, and we now see the reasons. On the one hand, the cultural backgrounds are different; on the other, we are dealing with a different *type* of unemployment, resulting from different causes, occurring in a different setting, and therefore calling for different adjustments. The traumatic effect is, however, found in some occupations which have a higher prestige and stability.

All this can be put in yet another way. In families where there is a double conflict—male-society, male-female—we shall get cases of breakdown, but also cases of adjustment, as I said before. Now we may regard the adjusted group, if it has proceeded to the stage of new, reified institutions, as a small culture pocket or subculture within the larger culture. This subculture is disorientated with respect to the surrounding dominating culture. Accordingly there will be very powerful resistances from both within and without the group, and these will show themselves in various attitudes.[2]

Let us now take the further step and study an example of such resistances, as reflected in typical attitudes.

[1] There has been long-term male unemployment in Dundee since 1870, i.e. before there was any legislation to deal with it. Probably because there was no legislation, the 'kettle-minders' had to develop a pseudo-institution of their own.

[2] Cf. small religious sects—Mormons, Holy Rollers, etc.

There is a curious and interesting social distinction between mill workers and factory workers. To the layman there does not seem to be a very strong reason for this. The working conditions and hours are much alike. In both cases the air is filled with dust and noise. Wages are practically identical. Yet a mill hand will say of a colleague who has taken work in a factory, "she has gone over to the other side" (i.e. she has gone up in the world). An important part of the explanation must be sought historically, and the pursuit of the history of this social attitude has led to some interesting discoveries for whose description the space of this paper is too limited.

This mill-factory stratification is already reflected in the vocational preferences and expectations expressed by children about to leave school at the age of fourteen. One example will suffice to illustrate the point: of the 478 girls about to leave school who answered the question, "What kind of work would you most like to have?" 21 (4·4 per cent) said "mill", 128 (26·8 per cent) "factory".

These figures provide some measure of the differential social values attributed to occupations which in the economic sense are very similar. They become even more surprising when we reflect that some 30 per cent (estimated figure) of the female working population actually is in mills. That is, though only one girl in twenty-five "would like to work" in a mill, one in three will ultimately do so, whether she likes it or not.[1]

Having in mind the general conclusions discussed above and this fact of the different social values attached to mill and factory work, I asked a factory manager about it. He at once said that he knew all about this belief, and that it was not merely a belief, but an incontrovertible fact. Pointing to his 600 girls, he said, "These girls all come from a much higher social class than the girls across the way (in a mill)." I asked him under what conditions they lived at home, what sort of houses they inhabited, and where. His reply was, "They all come from the three- to four-rooms-and-bathroom type of flat."

A statement like that is easily checked. In this particular city, the only thing the manager could mean by "three- to four-rooms-and-bathroom" is a corporation house. But in fact the mill and factory workers have the same sort of environment. They all live in tenements, and there is here a very sharp distinction between tenements and corporation houses. Their geographical distribution, to begin with, is quite different. The tenement dweller as a social type includes both mill and factory workers, and as soon as one gets out of the tenement class one is in the artisan, skilled worker and small tradesman class.[1]

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[1] A full analysis of the first and alternative vocational preference, and of the leisure activities of various groups, for 1000 school leavers, will be published later.

But, as further questioning showed, the factory manager was convinced that his girls were the social superiors of mill girls. This attitude is quite common among the better situated who should be in a position to know the real facts. We find a closely similar attitude towards the existence of slums. People who pass through them every day on their way to the office do not notice them, or minimize their bad appearance and effects, or express a rationalized attitude by arguing that only a very definite, restricted subhuman class lives there. (Cf. Edwin Muir (21), pp. 140ff.)

Stated in psychological terms, these people have developed a functional anopsia with regard to the conditions obtaining in other parts or groups of their social environment. This functional anopsia, like any other functional neurosis, is the result of attitude conflicts, in this case between groups as well as within the individual, and the history of these conflicts can be examined from several points of view.

All this is connected with our original starting point: the restriction of girls' vocational preferences and wishes. In any male-dominant society, women are restricted to certain functions and occupations. We can see in this particular case how the restrictions are made to operate automatically through the elaboration of certain social fictions, or through the rationalization and conventionalization of modes of procedure that for some reason or another have become institutionalized in a culture. Here, economic necessities beyond the knowledge or control of the individual worker forced female labour into certain categories. Now we find these categories accepted as biological necessities (weaving skill is inherited by *girls* born in 'weaving families'). Psychologically, this is reflected in the vocational interest restrictions of girls. Other effects have led to the growth of rudimentary 'pseudo-institutions' such as 'kettle-minding'.

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[1] This paragraph could be very much expanded. One wants to know in more detail, for instance, what the location of different tenements is, how rents are distributed within and outside the tenement class, what the ethnological distribution and history are (Polish and Irish immigrants at the end of the nineteenth century). But these details are not at the moment required. We are merely concerned with the *fact* that now (in 1936) many people, and even factory managers who might be expected to know better, are of the opinion that factory workers have a higher social standing and way of living than mill workers. For the distinction between palaeotechnic and neotechnic housing aspects of cities, cf. Patrick Geddes (10).

(2) *Conclusion*

The proposal is not new that in social psychology we should use economists and sociologists as 'consultants'. For instance, in the foreword to the volume entitled *After the Shutdown*, by Clague, Couper and Bakke (6), it is stated that "in both parts of the study, the investigators had the advantage of counsel from an advisory committee consisting of a political scientist, an economist, a psychologist, an industrial engineer, and two sociologists". Cf. also (8), (16), (20), (25).

But in this paper it is proposed that the economist and sociologist should be called upon as more than consultants: as fellow-inquirers and fellow-students. The specialists in the various other social sciences should work *simultaneously* with the psychologists; discuss the various problems that continually arise; and, most important of all, sink their particular sectional prides and preconceptions so that a common point of view, a uniform method, and a consistent terminology develop. In order to make these possible, a special effort is required and a willingness to learn from the other sciences, but more particularly an ability and readiness to take severe criticism in a constructive, not a defensive spirit.

Conferences on the social sciences and their interrelations are very instructive. Usually each specialist is determined to air his particular point of view or grievances. It is significant that at such conferences some speakers invariably say "Psychology *ought* to study this or that". In the team work we are advocating and attempting here, that attitude disappears. Instead, we get an instrument of research having real power of penetrating our complex industrial society. Whether or not the results obtained so far are new and significant seems of little moment compared with the advance achieved in the technique of co-ordinated field work.

The theoretical point which emerges from the discussions in this paper is that if we accept the delimitation of the field of social psychology implied in the definitions of paraphenomena and paraphenomenal behaviour, the various fields of sociology and economics are, broadly, also fixed. The latter are the sciences which aid the social psychologist in finding, accounting for and measuring his *stimulus* terms. That is to say, sociology is concerned more with the study of institutions and their paraphenomena in and for themselves. Economics is concerned with the laws of some very special paraphenomena, and in the study of these has made such advances that it is even able to put some of its methods and conclusions in mathematical symbols. Social psychology, on the other hand, is concerned with the study of the behaviour of men under the stimuli of paraphenomena.[1] To lay down by definition what the respective fields of different sciences are is always difficult. But a broad distinction is both possible and desirable. At one recent conference on the social sciences it was said that studies of the kind here advocated are "nothing but sociology". If we regard the problem from the point of view of stimuli and responses, the confusion disappears. There are fields about which it is hard to say whether they are part of physics or part of chemistry. Similarly there will be fields about which it is hard to say whether they are 'legitimately' the study of social psychology or of sociology; but that does not mean that we need quarrel about them. Call them, if you will, 'sociological psychology' or 'psychological sociology'; but let us get on with empirical research.

SUMMARY

The first part of the paper describes an attempt now being made to bring about a fresh approach to field work in social psychology. Definitions are given of *paraphenomena*, *paraphenomenal behaviour*, and *autogonous laws of institutions*. It is maintained that the stimulus terms of social psychology are described by sociology and economics, whilst anthropology contributes to the methods of field observation.

It follows that if a social problem, for example unemployment, is to be studied, a *team* of specialists must co-operatively analyse the whole field. One such team and its procedures are described and discussed. The basic method is that of *functional penetration*.

In the second part some specific findings are analysed. It is shown that vocational guidance on a large scale for industrial purposes needs to be based on field work of the type here described. Preliminary studies are reported of investigations into social types and the restriction of juvenile wishes by cultural fictions and vocational expectations.

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[1] Some writers distinguish between social psychology and sociology by reference to the different attitudes with which the respective subject matters are approached. Thus R. M. McIver ((18), p. 65): "What is it on which our attention is focused? If the nature of social structures, as created by and as fulfilling men's needs and purposes, then we are sociologists. If the nature of mind as revealed in the structures which they have built, then we are psychologists. It is a difference of attitude in regard to a common material." With this I agree, but maintain that as a distinction it is not exact enough. It is at once too broad and too narrow.

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SPECIFIC CONSCIOUS INTENT AND ITS CORRELATES IN PERFORMANCE

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SEVERAL investigations into distraction effects (1, 5, 6, 7) have called attention to their individual influence upon performance. This phenomenon has been studied by the writer in conscious activity (2, 3) through the investigation of the related changes in conscious intent and in performance during changing environmental situations.

I. THE PROBLEM

The present investigation deals with these related effects when the changing environmental stimulation is in the form of different instruction situations. Related changes in the instruction situations, in conscious stimulation or intent and in performance were sought in order to identify the causative factors influencing performance. Particularly, the experiment was planned to distinguish the influences upon performance of new specific intents while performance is motivated by a general intent to work at a uniform rate. This was accomplished by establishing the general intent in control tests and comparing any changing rates with changing instructions and changing intents reported in introspection.

(a) *The experimental situation*

The experiment consisted of twelve tests occupying approximately 1 min. each, which were administered individually and consecutively during one period of work of about 1½ hours. The observers dictated an exhaustive introspective report of conscious activity following each test. The introspections were entirely undirected by the experimenter. The task was the author's Speed Addition Test,[1] which consists of 2400 addition problems with totals of less than nine, in which the problems are distributed in chance order and arranged in rows of thirty, broken by a space between tens.

'Comfortable Work Instructions' were administered with the first test, as follows:

> Think of this as a mental work situation which you are paid to do and which may be a part of your life work for a great many weeks, perhaps years. You might think of yourself as working in an office and you do this task several hours a day. You plan, of course, to work at a normal or comfortable speed and to achieve the greatest satisfaction from doing the task. Choose the pace that gives you the greatest ease in work and maintain this same pace throughout all of the tests which follow. The first test will be a period of choosing the pace. After the first test you are not allowed to change in speed or rate of adding. You will add 120 problems. You will be told when to start. Indicate at the finish that you are through by saying 'Yes'. You will not know what is the time of your work from the beginning to the end of the series of tests in which these instructions are used.

Five tests were administered with these instructions to serve as a control series in which the objective conditions were established for performance with uniform intent. With the sixth test 'Warning Instructions' were administered, as follows:

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[1] Published by the Department of Psychology, New York University.

These instructions are similar to the last. You are to work at the comfortable pace you have chosen and continue this same pace throughout all of the tests of this observation. But you are given a word of caution. It will be difficult to follow these instructions, because many things will take place in the tests which follow to disturb this plan of work. Instructions may be given which you will wish to accept, but you must not do anything that will alter the comfortable pace of work which you have adopted. Follow these instructions until you are told that the day's work is over. Do not ask any questions.

In this way it was planned to continue the same general intent throughout six experimental tests in which the environmental situation was changing, in order to observe what related effect there might be in specific conscious intent and performance. The changing environmental situations consisted of new instructions placed before the observer without comment by the experimenter (Instructions A–E), and a final test (in some cases two tests) in which the instructions were to 'recapture the original rate' of work.

Instructions A. You have been working at a good speed, but you can do just a little more and still be comfortable. It is worth while trying. Other people have worked faster and still been comfortable. I am sure you can. It will give you some satisfaction to improve your score a little, because of the monotonous pace which you have been following. At least it will be a change in the day's work, which is always welcome. So speed up just a little and see if you don't like the new pace better.

Instructions B. You can find a more comfortable pace at which to work. Your pace is a bit too fast for you to have that pleasant dissociated attitude when the adding goes on as if you were asleep and you can allow day-dreaming or thinking of other things to have free direction. Slow up a little until you find this comfortable pace of dissociated work.

Instructions C. Things are not going well this morning. You do not follow instructions. We will make an entirely new start for the day. We have failed to do anything worth while thus far and we will change over to the instructions to follow the old accented rhythm with which you are very familiar. You are to beat out the time of the adding in time to the two-accented rhythm and I will set the pace so that the work will go on comfortably. In this way we will get that comfortable pace in which extraneous thinking can lend its pleasantness to the task.

Instructions D. As a final test we will attempt to break all previous records. Get a motor set for the task. Call on the organic factors to help. Watch your speed carefully to see that you are doing your best every second of the time. This is your last opportunity to break that one record which has been set as the apex of speed in adding.

Instructions E. Now I am going to help you in making one more final effort to break the high record. I will suggest when you are to go faster as in the similar experiment some time ago when we were trying to break records. Accept these suggestions and get started at the maximum speed possible.

(b) *The observers*

Six trained observers were selected for the experiment.[1] Their training consisted of (1) previous practice in the performance of the task, (2) previous practice in acceptance of the experimental instructions (Instructions A–E), and (3) experience in reporting dynamic conscious activity.

The amount of previous practice in the performance of the task of the experiment and the highest rate previously achieved in problems per minute is shown in Table I for comparison with the amount of additional practice and the highest rate in problems per minute in this experiment for each of the six observers. These figures show that the observers were familiar with the task and indicate that practice and fatigue were inoperative as influences upon the results of this experiment.

Table I

	Minutes of practice		Rate in problems per minute	
Observer	Previous to this experiment	Additional for this experiment	Previous highest	Highest in this experiment with 'Comfortable work'
A	786	11	169	130·4
E	750	14	133	104·8
F	95	16	116	88·3
G	97	16	138	104·6
H	89	18	161	83·9
J	78	24	108	73·7

The new instruction situations were familiar to all observers. Instructions A suggested an increased speed for greater comfort and Instructions B suggested a decreased speed to secure a dissociated attitude. Instructions C included a double-accented rhythm of a different rate from that at which the observer was working in his previous test with instructions to follow it. All observers had co-ordinated their rate with this timing in a previous experiment, with a low degree of accuracy for Observer A and absolutely no accuracy for Observer J. For this reason Test IX, in which these instructions were administered, was omitted with Observer J. All observers had previously worked continuously with an intent to beat their highest record (as directed in Instructions D) and all but Observer J had tried to do it with speed suggestions from the experimenter (as in Instructions E). By using these familiar situations with instructions which the observers had intended to follow in the past it was thought that a maximum influence would be exerted upon the observer's conscious activity in the development of specific intents to control performance according to the original intent to work at a comfortable pace without change.

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[1] The experiment was performed in the Psychological Laboratory of the University of Cambridge, England, in 1931–2. Acknowledgement is made of the assistance of F. C. Bartlett, J. M. Blackburn, K. Edwards, G. C. Grindley, M. D. Vernon, and P. E. Vernon of the Cambridge Laboratory, who were the observers in the experiment.

II. PERFORMANCE RESULTS

Performance results are shown in Table II by tests and observers, in which the upper figure, where two are shown, is the number of problems performed by the different observers in 1 min. The differences between the observers in level of performance in comfortable work is indicated by the S.D.s in the last column to the right (upper figure).[1] An average of these S.D.s for the twelve tests is 19·3 problems per minute. It is necessary to remove the influence of levels of performance and give the individuals a common denominator for the measurement of changes in performance. To do this, the performance results are reduced to percentages of Test II by individual observers, and they are shown as the lower figures in Table II. When this is done an average of the S.D.s for the various tests is 4·40 per cent of problems per minute.

The S.D.s for individual observers range from 4·48 to 10·73 per cent of problems per minute as shown in the last row at the bottom of Table II. An average of these observers' S.D.s is 6·61 per cent of problems per minute. In a previous experiment (4), where standards of variability with uniform intent were sought, and in which the subjects worked with the same task and similar 'Comfortable Work Instructions', the average of the S.D.s of twelve observers in eight tests was 3·68, which is an average of three successive periods of work. It appears that individual variability in this experiment is about double what has been established previously in work with uniform intent, and the cause of this increased variability is the problem of investigation in this experiment.

III. CONSCIOUS ACTIVITY DURING CONTROL TESTS

The observers were instructed to set their rate in the first test and introspections following this test tell how they intended to do it.

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[1] All standard deviations reported in this investigation are sigma of the distribution.

Table II. *Original data, showing problems per minute (upper figure) and percentage of Test II (lower figure) by observers and instructions (extra tests are in parenthesis)*

Test no.	Instructions	Observers A	E	F	G	H	J	Mean	S.D.
I	Comfortable work	133·3	102·0	—	91·7	93·6	62·2	96·6	—
II	Comfortable work	126·8	104·8	89·5	99·1	83·9	65·9	95·0	18·8
		100·0	100·0	100·0	100·0	100·0	100·0	100·0	0
III	Comfortable work	126·8	104·3	86·2	99·6	82·9	71·9	95·3	17·7
		100·0	99·52	96·31	100·5	98·81	109·10	100·71	3·99
IV	Comfortable work	125·0	100·7	86·8	102·8	83·0	66·3	94·1	18·4
		98·58	96·09	96·98	103·73	98·93	100·61	99·15	2·50
V	Comfortable work	130·4	100·0	88·3	104·6 (100·0)	80·3 (80·5)	73·7	96·2	18·6
		102·84	95·42	98·66	105·55	95·71	111·84	101·67	5·82
VI	Warning	126·8	91·1	84·1	103·2	79·2	66·0	91·7	19·31
		100·0	86·93	93·96	104·1	94·39	100·1	96·58	5·56
VII	A	138·5	87·3	82·0	98·4	76·6	68·7	90·3	24·67
		109·2	83·30	91·62	99·29	91·29	89·07	93·96	8·27
VIII		112·5	—	81·9	102·1	79·8	69·5	89·2	15·74
		88·72	—	91·51	103·3	95·11	105·5	96·83	6·54
IX	C	112·0	90·0	77·6	86·2	73·9	—	87·9	13·34
		88·33	85·88	86·70	86·98	88·07	—	87·19	0·90
X	D	162·2	90·7	77·4	90·5	81·3	64·3	94·4	31·60
		127·9	86·55	86·48	91·32	96·89	97·57	97·79	14·16
XI	E	—	83·3	74·7	—	72·1	62·2	73·1	7·52
		—	79·48	83·46	—	85·93	94·38	85·81	5·45
XII	Recapture original rate	121·4 (125·2)	90·2	76·9	95·1 (97·0)	—	68·2	90·4	18·21
		94·95 (98·74)	86·07	85·92	94·94 (97·88)	—	103·49	93·07	6·57
Tests II–XII incl.	Mean	128·2	94·2	82·3	98·2	79·3	66·7	91·5	19·3
		101·05	89·92	91·96	98·97	94·51	101·16	96·26	4·40
	S.D.	13·55	7·15	4·88	5·64	3·76	4·21	—	—
		10·73	6·82	5·43	5·77	4·48	6·40	—	—

(a) *Intent in initial test*

The reports of intent during the first test show that the observers were focally aware of setting a comfortable pace. All introspections include focal awareness of the sentences:

> The first test will be a period of choosing the pace. After the first test you are not allowed to change in speed or rate of adding.

Abstracted introspective reports of Test I follow:

> Observer A reported that he "was aware of smoothness of adding; was watching the process carefully, and there was a doubt if I could reproduce the speed being set". During this first test Observer E, "tested various speeds, started faster and slowed down a bit. In last line was restraining myself to keep a comfortable pace." Observer F, "verbalized the totals". Observer G, "verbalized the numbers and intended to do each ten problems as a unit and at a steady pace. Was restraining myself. Aware of smoothness and of pencil." Observer H was aware of "trying to secure definite pauses between each problem and a certain amount of pride in writing movements. Irrelevant imagery was present." Observer J, "tried to settle down to a comfortable rate, thinking about the speed. Toward the end there was irrelevant thinking."

In this manner the intent was centred upon controlling the speed of work during the first test. While doing the task was highly focal, there was some irrelevant thinking. Sensitivity to movement, steadiness, smoothness and so on, which were largely visual, and verbalizations of the sums, appeared to be the essential cues in rate guidance. In a previous study (unpublished) of verbalization with these same observers, it was found that all observers verbalized the answers consistently, but with varying degrees of subvocal speech under normal conditions. With this evidence it would seem that all of the observers must have used verbalizations in rate guidance, while only Observers F and G reported awareness of it. There was little evidence, except in doing 'a unit of ten' and securing 'definite pauses', of the use of quantitative cues in the guidance of rate.

(b) *Intent in control tests*

In the 'Comfortable Work Instructions' the initial test was to be one of choosing a pace, after which there was to be no change in rate of adding. Abstracted reports of intent in the four control tests (Tests II–V incl.) are given by observers.

All awareness reported by Observer A was related to the instructions or task, and intent was highly specific in the selection and rejection of methods of controlling speed.

Observer A reported that during Test II, "I was principally aware of kinaesthesis in my arm as if it were being held back" and "performance was interrupted and not smooth". In the third test there was awareness of "a speed drive trying to work its way through. I let it go in the formation of the figures, but I superimposed upon the speed of work a certain restraint." The observer thought of articulating, feeling that previous oscillations in speed might have been due to the fact that he hadn't. In the fourth test he was "aware of the adding motor process of my arm holding me up (in kinaesthesis)" and "all awareness is on keeping the speed right. There is something holding me back." There was a similar report in the fifth test and he said, "I judge speed from the rate the pencil is moving and on whether the work is smooth or interrupted. If going too slow I try to go faster at the end of the line; if too fast I try to restrain it a bit. This kind of awareness is superimposed upon the doing of the test."

Observer E developed a rhythm to regulate speed, but did not use it throughout all the control tests in checking his rate.

Observer E reported irrelevant thinking in the second test and said, "I was aware of going faster when not attending to the speed. When I attended I restrained myself slightly. I didn't attend to this as much as in the first test. The restraining feeling was mental, i.e. I thought 'I can do this much faster, no use hurrying, etc.'." In the third test he reported, "I was concentrating more on a rhythm of intervals by movement of hand, which was kept up by thinking, 'Tum Tittle Tum', and there was vague awareness of the speed." He was aware of hurrying when not aware of the rhythm. In the fourth test he said, "I tried to counteract a tendency to go faster. (In this test he reduced speed about 4 per cent.) I challenged myself about various things. The 'holding back' was verbalized in the form of a statement, 'Don't go so fast'." In the fifth test he said, "I kept old speed. It is a feeling that the rhythm I have is the same rhythm I had before. No way I can describe rhythm. Irrelevant thinking comes in a bit."

Observer F verbalized the totals throughout all tests and reported irrelevant awareness which was concerned with the expected stimulation.

In the second and third tests Observer F "verbalized the totals" as in the first test. There were irrelevant awarenesses. In the fourth test there was "awareness of a tendency to speed-up all the time. Think my time is not constant, varying a bit. Method is same. Keep regular speed by writing speed." In Test V he reported, "verbalizations regarding speed as before. I do a set occasionally without this, but just verbalize the sums. There is irrelevant thinking about it all."

Observer G intended to relegate the guidance of speed to the automatic and did so, continuing the task in a dream state.

In the second test Observer G reported a "verbalization (not vocalization) of the figures to keep them at a steady pace" as in the first test. There was "a tendency to speed-up and an intent to keep it steady". There was "awareness of pace in relation to first test". In the third test he was "less aware of adding process, but more of trying to keep steady. There is irrelevant thinking of object of experiment, and that the adding process is getting automatic and I shall be able to do it without thinking of speed." The fourth test was "a comfortable process; was aware of smoothness. I was aware of task a little as I had a slight feeling of dismay at times that I might take too much time on a figure. Irrelevant awareness came in the latter part of test and I lost awareness of task altogether." (Here the observer's score increased 3 per cent.) In the fifth test there was "awareness of the movement of the hand in a vague way, otherwise there is steady motion and the adding seems to go on of its own accord. (Here the observer increased 2 per cent in score.) Otherwise my mind seemed almost a blank."

Observer H depended upon the original speed set to guide the rate established in the first test. Irrelevant thinking was prominent.

Observer H reported in the second test awareness "chiefly of speculation as to object of experiment and thinking that there would be some quickening—that there would be some practice effect. There was comparisons with speeds at various times." In the third test he reported an "awareness about how difficult it would be to keep this up over a period of time and if there could not be some form of day-dreaming that would make it more pleasant". He reported, "not troubling about the speed so much, but endeavouring to keep in the same set. It is impossible to memorize the speed kinaesthesis." In the fourth test he reported irrelevant thinking, "wondering if there is a systematization of groups of fives" and that there is a "slight tendency to a hypnotic feeling". In the fifth test he was "aware of verbalizing the answers", which, he said, "I was not aware of before, but think I must have been doing". There was much irrelevant thinking and "I tried to decide if I was increasing or decreasing speed, but didn't do anything about it".

Observer J fluctuated the most of all the observers in his speed in the control tests and it is evident that there is very little conscious focalization upon the rate of work. The intent throughout was to let the rate continue as before.

Observer J reported in the second test, "just occasional thinking about the task. I thought at the beginning, 'Now he wants me to do this at the same rate and I wonder if I have any internal rhythm by which I am timing myself', and I decided 'No', that the way I was doing it was by working at a comfortable rate. There was much irrelevant thinking about a party." In the third test it was reported; "That time I started in same fashion as previous time. Then it suddenly occurred to me that I was going faster and slower. This started me thinking of fluctuations in mental work curves. I had no intent to change the rate but to let it go at a comfortable speed." In the fourth test he reported; "I was getting bored with it and I thought, 'I wish I had finished this'. I tried not to let it interfere with my speed and I verbalized 'I must not let it interfere with my speed'. I had no intent to go faster or slower. My whole attitude in all tests is to let myself go on and not direct my speed." In the fifth test there was irrelevant awareness; "I thought, 'I must just let myself go on adding'."

These introspections of intent during the control tests show that there was a continual checking of speed by Observers A and E, some by Observers F and G, and relatively less by Observers H and J. Much of this was in the form of subvocal speech. Typical of Observer J was the report, "I had no intent to change the rate but to let it go at a comfortable speed". Observer H endeavoured to keep the motor set established in the first test and he thought of memorizing speed kinaesthesis. It is interesting to find that Observer H became aware of verbalizing the totals at the fifth test, and thought that he must have been doing this all along, although he reported "no articulations" in the first test. Observer F allowed verbal-motor timing to regulate the rate throughout, which was his method in the first test. On several occasions after reporting, "verbalizing the totals", he stated, "That is all" or "The same method", implying that this cue was focal throughout. Observer G used verbalizations up to the third test, but by the third test this observer was less aware of the task and the process had become fairly automatic. Only with this observer did the rate-setting cues become progressively vague in awareness. But they were always vague with Observer J. Observer E reported the development of a rhythm, but it soon got vague, although he thought he was following it. Rate cues were most focal with Observer A, but there was a motor set, as "speed driving to work its way through". Irrelevant thinking was present in the tests with all observers.

IV. PERFORMANCE IN CONTROL TESTS

Variability in the control tests (Tests II–V incl.) is shown in Table III in number of problems per minute and in percentage of problems per minute. The average S.D. variability for the six observers during four tests is 2·35 per cent of problems per minute when the average rate is 95·2 problems per minute. Variability between observers is represented by a S.D. of individual observers' S.D.s of 1·30. Hence, the S.D. variability, with uniform intent as indicated in the introspection, is 2·35 ± 1·30

Table III. *Variability in control tests shown in problems per minute and in percentage of Test II*

Observers	No. of tests	Problems per minute		Percentage of Test II	
		Mean	S.D.	Mean	S.D.
A	4	127·3	1·96	100·36	1·55
E	4	102·5	2·12	97·76	2·02
F	4	87·7	1·29	97·99	1·44
G	4	101·5	2·28	102·45	2·29
H	4	82·5	1·34	98·36	1·60
J	4	69·5	3·41	105·39	5·18
Mean	4	95·2 (100·3)*	2·07 (1·80)*	100·39 (99·38)*	2·35 (1·78)*
S.D.	0	18·3 (15·56)*	0·71 (0·41)*	2·77 (1·79)*	1·30 (0·32)*

* Results for Observers A–H incl. in parenthesis.

per cent of problems per minute over four tests. Without the record of Observer J this mean S.D. would be reduced to 1·78 with an S.D. of the distribution of observers' S.D.s of 0·32.

(a) *Comparison with standards of variability*

In a study of variability of performance with uniform intent (4) the average variability shown in Table IV was found for twelve subjects in the same tests (Tests II–V incl.) for four different periods of work. The

Table IV

Period of work	Mean of subjects' S.D.s	S.D. of subjects' S.D.s
I	4·36	2·07
II	3·14	1·88
III	3·37	1·23
IV	2·51	0·58

subjects in this earlier experiment had had no training in the task prior to the first period of work, as had the observers in the present experiment. Variability during the control tests of the present experiment was no higher than these standards of variability with uniform intent after training (during the latter periods of work), which suggests that the control test results in this experiment are representative of a psychological situation in which uniform intent is directing the rate to a high degree.

The amount of average changes in percentage of problems per minute from test to test for the six observers in the four control tests is shown in Table V along with the same changes for the four periods of work, taken from the experiment referred to above (4). These observers changed their performance from the previous test on the average 3·07 per cent (absolute average change) at an average rate of 95·2 problems per minute, with a S.D. between them of 2·96. The absolute change from Test II was similar. The results indicate again that variability, as shown by amount of average change from Test II or from the previous test, was well within the standard. That five of the observers (Observers A, E, F, G and H), omitting Observer J, offered more homogeneous results has already been indicated. The amount of average change for these observers is shown separately in Table V, and it is considerably lower than when Observer J is included. These observers changed their performance from the previous test on the average only 1·76 per cent (absolute average change) at an average rate of 100·3 problems per minute with very low variability between observers (S.D. = 0·20).

Table V. *Average change in percentage of problems per minute for control tests (Tests II–V) compared with standard change*

	All observers		Observers A–H incl.		Standard change for four periods of work	
Various averages	Mean	Observers S.D.	Mean	Observers S.D.	Periods	
Arith. av. change from Test II	+0·51	3·69	−0·82	2·38	I	+1·7
					II	+3·4
					III	−1·5
					IV	+0·04
Arith. av. change from previous test	+0·56	1·94	−0·12	1·33	I	−0·5
					II	+0·5
					III	−0·5
					IV	−0·2
Absolute av. change from Test II	3·28	1·84	2·51	0·65	I	7·0
					II	4·7
					III	4·7
					IV	3·3
Absolute av. change from previous test	3·07	2·96	1·76	0·20	I	5·3
					II	4·3
					III	5·3
					IV	3·6

(b) *Comparison of variability and intent*

The observers have been rated upon the degree of focus of cues used intentionally in rate guidance during the control tests. A rank difference correlation between magnitude of this rating and magnitude of variability (in reverse order) is +0·84 ± 0·08. Between ratings of frequency, or number, of possible cues in awareness during these tests and magnitude of variability (in reverse order), the correlation is +0·12 ± 0·27. These are rough measures, where the introspective data are quantified, and they have little statistical significance. But they imply that conscious focus of cues used in rate guidance is related to low variability in performance under the conditions of the control tests, while frequency of conscious cues is not so related.

The experiment referred to above (4), in which the standard change in performance was secured, established the same intent as in this experiment to work at a comfortable rate. But in the present experiment additional intent was present, after the first test, to maintain rate without change. It seems probable that conscious guidance of rate may have been the cause of the lower variability and the lower amount of change in this experiment. Also, when Observer J is eliminated, there is greater homogeneity of results and the remaining five observers (Observers A, E, F, G and H) have considerably lower variability and change in performance, as shown in Tables III and V. With Observer J, rate guidance was definitely less focal than with the other observers, which is further evidence for the conclusion that the lower variability in the control tests of this experiment may have been due to greater conscious guidance of rate.

In the comparison of the introspective reports and variability in the control tests it is suggested that low variability is found where there is a consistently focal intent involving efficient cues for rate guidance. Where intent is variable or changing in conscious focus there is greater variability of performance. Observer F, who ranks lowest in variability, maintained a consistently definite intent throughout the control tests. Observer A, who ranks second, had what would seem to be the clearest detail in intentional activity. Observer H held to a consistent intent, although somewhat vague in report, and he ranks a close third in low variability. Seldom was intent focal with Observer J and never was it clearly defined, and he ranks highest in variability. When Observer E's intent changed, variability increased. Also, when Observer G's intent became vague, variability increased greatly.

V. Intent in experimental tests

The purpose of the experimental tests, once a control situation of performance with uniform intent had been established, was to observe what influences there might be upon intent and performance. The 'Comfortable Work Instructions' stated that a pace should be 'chosen' in the first test, after which the observer was 'not allowed to change in speed or rate of adding'. All observers tried to follow these instructions during the control tests, though with varying consistency and methods. The 'Warning Instructions' added the statement to 'follow these instructions until you are told that the day's work is over', and this was accepted by all observers, with the exception of Observer A, as indicated by the introspections following each test.

(*a*) *A changing intent with new instructions*

Observer A adopted an intent to follow the new instructions of each experimental test. His performance, as indicated in Table II, conformed to his intent.

In Test VI, with the 'Warning Instructions', Observer A reported "extraneous thinking, fairly automatic work, no articulations, not aware of trying to decrease or increase speed" (percentage of Test II: 100·0). In Test VII, with Instructions A, the observer adopted a questioning attitude and reported "My intent at the start was to increase the working speed. I took this because of the instructions. As I tried to increase my speed I was aware I had done the same in previous attempts. At the last line I realized I was going a little slower and articulated to accelerate. I was principally aware of speed in the way my pencil and hand moved and began by working at a speed that fitted in with previous higher speed movements" (percentage of Test II: 109·2). In Test VIII, with Instructions B, the observer reported that his intent at the start was "To take it easy and arrange the test so that working conditions were comfortable and to carry on with day-dreaming". He said he was aware principally that he was going "at a very comfortable speed which was very much slower than the previous test". He knew this by "greater pauses between groups of 10 and other intervals". There was a more "casual set" but he was not sure of any day-dreaming (percentage of Test II: 88·72). In Test IX, with Instructions C, the observer reported confusion. His intent at the start was "to work at a comfortable pace to the accompaniment of sound" but "didn't get started any too well. Then found bits of rhythm that I could follow and that was a struggle between two rates" and "plans changed several times". There was kinaesthesis in hand. "I wanted to go along quickly but was being restrained." The rate of accented rhythm to problems performed was $\frac{114}{120}$ which is objective evidence of the conflict reported. Thus, he did not follow his intent exactly, but in a trial of ability to follow auditory timing it was found that he could never do it exactly as could other observers, and this was probably as close as he could come to equalling an objectively determined rate (percentage of Test II: 88·33). In Test X, with Instructions D, he reported an intent at start "to break his record", which was 169 problems a minute, and he accomplished 162·2 problems a minute in this test. He reported a "feeling of unreality and unsuitable conditions for the attempt. Can't recall any awareness except an unsatisfactory spell in the second line" (percentage of Test II: 127·9). Instructions E were not administered to this observer and he reported, when instructed to 'recapture previous rate' in the first five tests of the day's work, an intent at the start "to get back to comfortable pace at which I was working at first" and that "this was a little slow" (percentage of Test II: 94·95). On a second try he reported, "don't know how close I was working to old speed, but from what I can remember I was working smoothly" (percentage of Test II: 98·74).

In Test VI, with 'Warning Instructions' Observer A reported extraneous awarenesses and increased automaticity in the task. There was no change in intent and performance did not change. Intent changed with Test VII and Instructions A, where a choice is indicated, and the observer's intent conformed to the new instructions in each of the following experimental tests. Upon noting Observer A's acceptance of the new instructions the experimenter introduced the question following each introspection, "What did you intend to do when you started this test?" Observer A's awarenesses were all related to the new instructions. There was awareness of the original intent to work at a comfortable pace without change. Intent was consistently focal, with definite cues to maintain rate according to each new instruction situation. There was little irrelevant awareness.

Performance, as shown in Table II, correlates almost exactly with intent or instructions. The rank difference correlation between performance and either estimated magnitude of intent or of instruction is +0·99, which gives a rough quantification of this relation. The causative significance of intent is indicated by the fact that this observer returned very closely to his original rate in the final test (Test XII) with instructions to recapture the original rate. Variability for this observer during the experimental tests is shown in Table VI where the S.D. is 13·77 per cent of problems per minute, which is 3·21 times the average for the other observers in these tests. This variability is 12·21 times the S.D. of this observer's control tests, when an equal number of experimental tests was used.

It is evident from this analysis that Observer A departed from the conditions of the experiment and his results cannot be used in the study of the effects with which this experiment was concerned. But his results are added evidence for the previous conclusion (2, 3) that conscious intent is the significant determiner of rate of performance.

(b) *Introspective study of observers with original intent to follow 'Comfortable Work Instructions' during experimental tests*

All the remaining observers (Observers E–J incl.) maintained a general intent to follow the 'Comfortable Work Instructions' of the control tests with variations in additional specific intents to meet the new situation created by the experimental instructions. Abstracted introspected reports by these observers for Tests VI–XII incl. will follow. Each observer was allowed freedom of description of conscious activity without comment, except that the question, "Is that all?", was used as a final remark by the experimenter.

Observer E, it will be recalled, had developed a vague rhythm to control speed, but conscious activity was focussed upon the determination of speed during the control tests.

Observer E "was keyed up with emotional excitement" during Test VI, when the 'Warning Instructions' were administered. He reported, "I was intrigued by the instructions. There was emotional feeling of excitement, which made me hurry faster than I had been going. It was difficult to counteract. I put on the brakes by saying, 'I must go slower for a bit'. I wasn't thinking about the speed as much as other factors. At the beginning I thought I was going faster, but couldn't do much about it because I was keyed up. Afterwards I settled down to normal speed." With Instructions A, in Test VII, this observer reported, "I intended to follow previous instructions and not to follow this in spite of its pursuasiveness. The sentences of Instructions A were in awareness and they amused me. There was vague voluntary control most of the time to check myself from going faster in form of a release order to my hand to write the next figure." Instructions B were omitted, and in Test IX, with Instructions C, when the ratio of accented auditory rhythm to problems accomplished was $\frac{150}{120}$ this observer reported, "I tried to cut out the rhythm of the bells and wasn't writing with them. Pace was irregular at first, then became regular, trying to get my normal speed. Bells were not entirely in background, but think they would be obliterated if I did the test again. Was emotionally keyed up and difficult to know if working at old speed. May have over-compensated." In Test X, with Instructions D, this observer reported, "a lot of different feeling there. Felt it a pity I couldn't try to beat my record. Tempting thought came in before test: 'Shall I disobey instructions and try to beat my record.' But I opposed this and tried to work at normal speed." There were "thoughts of not having another opportunity". In Test XI, with Instructions E, there was the report, "I was over-compensating trying not to follow your instructions and at times I realized this and I thought then that your suggestions of speed and my own suggestibility would counteract each other. The problem of maintaining the regular speed was highly focal and I had to pay too much attention to the adding to get a general idea of my rhythm." In Test XII, with instructions to recapture the original rate, this observer reported, "thoughts about changes in instructions, tried to get a comfortable speed and I allowed myself to write 'now, now, now', but not all of the time".

It will be recalled that Observer F consistently verbalized the totals throughout all the control tests.

Observer F reported, "nothing much" in Test VI, when the 'Warning Instructions' were administered. He was aware of "occasional slight hold-ups" and reported, "wondering what was going to happen in succeeding test". Quarter period records showed slight gradual slowing of performance from beginning to end, but the observer reported "I am getting more regular in my work." In Test VII, with Instructions A, the observer reported, "I was amused by the instructions and read them as if they had nothing to do with the experiment and during the early part of the test I was aware of them and recalled the phrase, 'Do not let anything that happens change your plan of work'." The observer stated that the "instructions didn't have anything to do with intent and belonged to same class of things as sounds outside the room". (Observer put instructions as far away as possible after reading them and before beginning work.) He stated that "the work was a trifle slower, due to correcting three errors"; and again, that he "may have been a trifle slower as reaction against instructions". In Test VIII, with Instructions B, the observer reported an "attitude of doubt most of the time whether I was going slower, if I were gradually going slower from the beginning of the tests. The intent was the same." In Test IX, with Instructions C, the observer reported, "realizing how much slower I was going than before and a tendency to drop into time with the rhythm. At the middle of the test the rhythmic accompaniment went into the background and by the end it was shut out. Didn't follow rhythm at any time." The ratio of the accented rhythm to the number of problems is $\frac{144}{120}$. In Test X, with Instructions D, the observer reported that there was "no difficulty in getting rid of these instructions. I don't connect them with what I am doing. There were occasional irrelevant awarenesses about experiment and my speed, but very little else but verbalizing the sums." In Test XI, with Instructions E, the observer reported, "At the bottom line I got into wrong rhythm, somehow the last, of each group of two became the first. Think I may have slowed up as anti-suggestion and 'you' supplied the irrelevant awarenesses." In Test XII, with instructions to recapture the original rate, there was reported, "very little awareness extraneous to the adding. Occasional wondering if I had slowed up since the beginning. Definitely working in twos."

In the latter control tests, Observer G allowed speed to be regulated as it seemed to flow in automatic activity and worked in a day-dreaming state.

Observer G reported increased awareness "of process of adding and writing throughout the whole test" in Test VI upon administration of the 'Warning Instructions'. He reported thinking, "Be on look out, anything might happen". In Test VII, with Instructions A, he reported awareness "of smooth speed, am definitely regulating time intervals. This was about half and the other half was puzzling over instructions and wondering if I was doing things right." In Test VIII, with Instructions B, he was "aware of keeping a comfortable pace with regular steady pleasant working speed. Not sure how it compared with previous speeds and not directing speed as much as in last test. Most aware of puzzling over instructions and doubting about course to follow." But with Test IX, and Instructions C, this observer reports, "principal awareness of resistance to instructions and feeling that I had been 'taken in' by last instructions. At first, my thought was 'I am not going to follow these instructions and I will try to go at my original steady pace'. I was aware of doing one sum to each accent and then I went slower to get out of time with the rhythm. There was a struggle and no smoothness, and kinaesthesis in hand." The ratio of accented rhythm to problems performed was $\frac{146}{120}$, indicating no co-ordination between these two rates. In Test X, with Instructions D, this observer reported that he was "principally aware that he wasn't going to get caught by the suggestion to make a record. Was not aware of regulating speed but let it go at comfortable rate, thinking it would be same as original." Test XI, with Instructions E, was omitted with this observer and in Test XII, with instructions to recapture the original rate, he reported that he started with "awareness of going same speed as last test" which was followed by day-dreaming and irrelevant thinking. "There was no regulation of pace. Let it go at its own rate", which was the intent of the final control tests.

In the control tests, Observer H had established a set upon which he depended to regulate the speed.

Observer H reported in Test VI, with the 'Warning Instructions', "awarenesses chiefly of meaning of instructions, and what might happen, also increased awareness of everything, and wondering if expectant situation would have any effect on speed. Attended less to task." In Test VII, with Instructions A, the report was one of increased awareness of the situation, "wondering if I would see the rates later, if it would make me go slower than faster, theoretical speculations as to suggestibility of instructions to go faster and that experimenter was trying to make me go faster. Also, I was aware of intent not to compensate by going slower and tried to dissociate instructions." In Test VIII, with Instructions B, this observer "was trying to balance and compare speeds of morning. Decided I had perhaps slowed down a bit. Thought of speed of tests two or three tests back and tried to get it. Some speculations regarding effect of instructions" and irrelevant thinking. In Test IX, with Instructions C, he reported "increased verbalization to carry own rate. Was aware of difficulty of keeping pace and probability that I couldn't recall the previous pace and I was probably getting slower. Thought, in my attempt to overcome, that the pace might be faster. Some speculation as to rhythm but most awareness of task." The ratio of accented rhythm to problems performed was $\frac{188}{120}$. In Test X, with Instructions D, the observer was "aware of speculating as to best method of putting instructions out of mind or if thinking of it would give it a chance to act without intending to do so. Aware of slowing but no compensating intent that spread over any time." In Test XI, with Instructions E, the observer reported "increased awareness of speed, smooth writing, irrelevant awareness of effect of coaching, and that in rowing a contrary effect came in a third of the time. Aware of only slight variations in speed and there might be a tendency to go slower, because a figure was written too fast." Test XII was omitted with this observer. This observer summarized his intent, in the experimental tests, "to keep same speed, hoping that positive and negative factors would about cancel each other".

Observer J, it will be recalled, had allowed the speed to take its own course in a comfortable manner and there was little conscious activity of determining rate.

Observer J reported, "speculation of what might happen" in Test VI, with the 'Warning Instructions', and he said, "I was aware of a check at one or two places and my hand came to a stop". In Test VII, with Instructions A, the observer reported, "The result of these instructions and knowing that I wasn't to follow them caused me to go at the usual slow pace. Consequently throughout I was aware of the intent that 'I must not speed-up'." In Test VIII, with Instructions B, this observer reported, "awareness that the instructions were not very tempting, as my pace was already comfortable. No effort to go faster or slower and an attitude 'I mustn't go faster or slower, but go on adding at the easy pace'." Test IX, with Instructions C, was not administered to this observer as he could not co-ordinate his adding with rhythmic timing. In Test X, with Instructions D, he reported, "as soon as I read the instructions I verbalized 'I am not going to do any such thing'. Then I started at my usual plan of letting myself go. There was irrelevant thinking of meaning of instructions." In Test XI, with Instructions E, the observer reported, "There is a definite tendency just to hurry, much more marked than having read some instructions. I kept on saying to myself, 'Now I mustn't take any notice of what he is saying'. I let myself go on adding. There was emotional tension—more than before." In Test XII, with instructions to recapture the original rate, there was the report of "irrelevant thinking if my speed had decreased or increased throughout the experiment. I didn't know. There was no intent to change speed."

(c) *Summary of conscious activity during experimental tests*

The five observers (E–J incl.), who maintained the original intent to work at a comfortable rate without change throughout the experimental tests, developed new specific intents to keep effective the original intent.

These additional intents are recorded in the Appendix for comparison with performance. The amount of development of new specific intent is greatest with Observers E, G and H, and least with Observers F and J. When the recorded additional specific intents are compared with the abstracted reports, it will be noted that in many tests, and with Observer F in particular, interpretation seems to be based upon little exact reporting. It will be recalled that in all introspections the observer was allowed complete freedom of description in order to avoid the suggestion of cues with which the observer might be concerned either in maintaining the rate or in reporting. It has been necessary, for this reason, to include obviously implied intent, and for these interpretations the information derived from the observers' conscious activity in previous experiments has been a decided help. When there was a significant general change in conscious activity this also is indicated in the Appendix.

VI. PERFORMANCE IN EXPERIMENTAL TESTS

Fig. 1 shows the average percentage performances in all tests computed for Observers E, F, G, H and J in Table II. These are the five observers who maintained the original intent to work at a comfortable pace throughout the experimental tests. The figure shows that performance was maintained at the level of Test II for the control tests, but dropped considerably at Test VI, with 'Warning Instructions', and continued an irregular descent throughout the experimental tests with the lowest rates in Tests IX and XI with Instructions C and E, which included objective timing. The average for the control tests is 100·39 per cent of problems per minute and for the experimental tests 91·64 per cent of problems per minute. Fig. 2 shows the average variability for these five observers by tests. The average S.D. for the control tests is 4·47 per cent of problems per minute, with Test II omitted because variability is determined at zero in this test; it is 5·09 per cent of problems per minute for the experimental tests. While variability between observers does not change significantly from control to experimental tests it is evident that rate of average performance is definitely affected by the experimental situation.

Table VI gives results for the six observers in the experimental tests in which various groups of tests are combined to show differences in variability: Column A includes all experimental tests (Tests VI–XII incl.); column B omits only the final test in which observers were instructed to 'recapture original rate'; column C omits only the test in

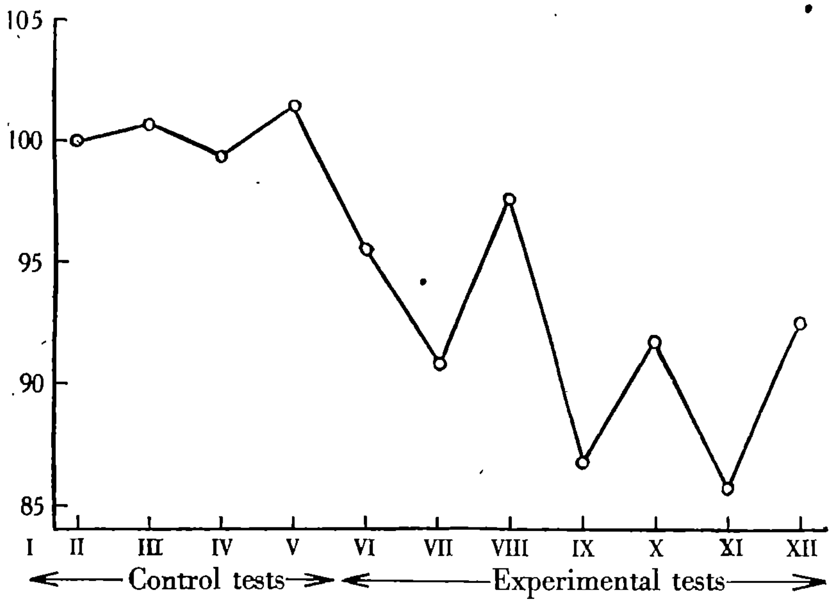

Fig. 1. Showing average rate in percentage of problems per minute for five observers (E–J incl.) by tests.

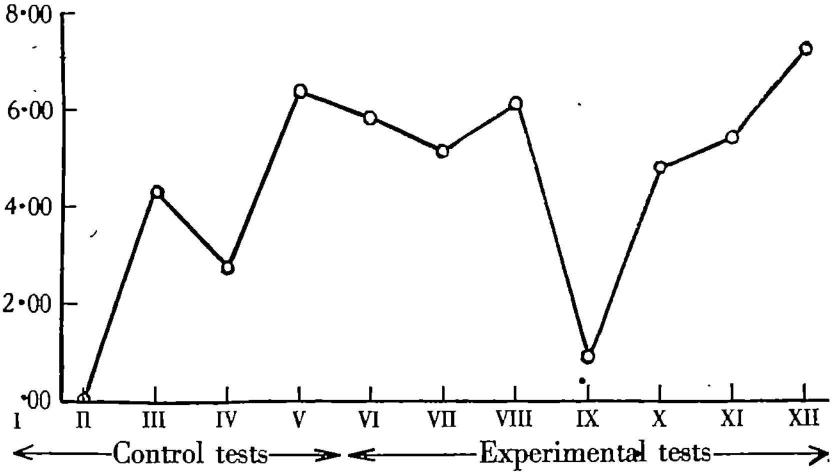

Fig. 2. Showing average S.D. of rate in percentage of problems per minute for five observers (E–J incl.) by tests.

which 'Warning Instructions' were issued; column D includes four tests only, the first four of each observer with Instructions A–E.

The results of column D offer a comparison of an equal number of experimental and control tests. Ratios of the observers' S.D.s of these experimental tests over the S.D.s of control tests are: Observer A, 12·05; Observer E, 1·39; Observer F, 2·19; Observer G, 2·80; Observer H, 2·58; Observer J, 1·15; mean for Observers E–J incl., 1·79. All observers increased variability and three of the five more than doubled their variability in the experimental tests.

Table VI. *Means and S.D.s in experimental tests shown in problems per minute and in percentages of Test II*

	A, Tests VI–XII incl.				B, Tests VI–XI incl.			
	Problems per min.		Percentage of Test II		Problems per min.		Percentage of Test II	
Observers	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
A	128·9	17·4	101·52	13·77	130·4	18·7	102·83	14·74
E	88·8	2·7	84·70	2·27	88·5	2·9	84·43	2·74
F	79·2	3·2	88·52	3·55	79·6	3·3	88·96	3·85
G	95·9	6·1	96·66	6·21	96·1	6·1	97·00	6·25
H	77·2	3·3	91·95	3·92	77·2	3·3	91·95	3·92
J	64·8	3·6	98·35	5·52	64·1	3·6	97·32	5·50
Obs. E–J incl.:								
Mean	81·18	3·78	92·04	4·29	81·10	3·84	91·93	4·45
S.D.	5·31	1·20	5·05	1·41	10·82	1·15	5·46	4·35

	C, Tests VII–XII incl.				D, four tests with Instructions A–E			
	Problems per min.		Percentage of Test II		Problems per min.		Percentage of Test II	
Observers	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
A	129·3	19·0	101·82	15·07	131·3	20·8	103·54	18·68
E	88·3	2·8	84·26	2·64	87·8	2·9	83·80	2·81
F	78·4	2·7	87·62	2·99	78·7	2·8	87·95	3·16
G	94·5	5·6	95·17	5·74	94·3	6·3	95·22	6·42
H	76·7	3·5	91·46	4·12	76·7	3·5	91·46	4·12
J	64·6	3·9	98·00	5·99	63·7	3·9	96·63	5·95
Obs. E–J incl.:								
Mean	80·50	3·70	91·30	4·30	80·24	3·88	91·01	4·49
S.D.	10·28	1·04	4·96	1·37	5·12	1·27	4·02	1·45

VII. COMPARISON OF VARIABILITY AND INTENT

Throughout the control tests all observers intended to work at the comfortable rate without change as directed in the 'Comfortable Work Instructions'. With the introduction of the experimental instructions, various changes in intent took place. Observer A accepted each new instruction and his performance is correlated exactly with his intended performance. The other five observers (E–J incl.) intended to work at a comfortable rate without change throughout the experimental tests. The rank difference correlation between the ratings of degree of focus of cues for rate guidance and variability (in inverse order) for the experimental tests is +0·10 ± 0·30, while this correlation for the control tests was +0·84 ± 0·08. But the correlation between the rating of degree of focus of cues in the control tests and variability in the experimental tests is +0·73 ± 0·14 which suggests that awareness may have been too indefinite in the experimental tests for adequate reporting of the cues of rate guidance to form a basis for the rating. The rating of frequency of cues in awareness and variability (in inverse order) in the experimental tests is +0·30 ± 0·27.

Additional specific intents developing in awareness during the experimental tests are compared in the table in the Appendix with changes in performance by individual observers. The amount of change from Test II and from the previous test is shown in the columns at the right in this table, where the ratio is this amount divided by the absolute average of the same changes for the control tests. By way of illustration, during Test VI, with 'Warning Instructions', Observer E decreased his rate by 13·07 per cent from Test II and by 8·49 per cent from the previous test. Divided by the absolute average change of tests from Test II in control tests, this change from Test II has a ratio of 4·3, which is a change that is 4·3 times the average change in the control tests. Divided by the absolute average change of tests from previous tests in the control tests, the change from the previous test has a ratio of 5·6, which is the same thing as saying that this change is 5·6 times similar changes in the control tests. Increases and decreases in amount of change are shown by plus and minus signs.

The percentages of the changes from Test II which are two or more times the average for the control tests are for the different observers:

(2) Observer E, 100 per cent,
(4) Observer G, 33 per cent,
(1) Observer F, 100 per cent,
(3) Observer H, 83 per cent,
(5) Observer J, 0 per cent.

The figures in parenthesis give the rank from low to high variability for these observers in the control tests. These figures indicate that the total effect of the experimental instructions upon rate was most for those observers with lowest variability in the control tests. It will be recalled that Observer J's variability in the control tests was three times the average of the other four observers and his changes in rate in relation to

Test II were about the same as those in the control tests. For the other observers there was a general tendency to decrease rate, which accounts for the average decrease throughout the experimental tests in Fig. 1. But while there was a tendency to decrease as the experimental tests progressed it is noted that the rates for the various observers went up and down in irregular fashion in so far as Test II is concerned.

This irregularity is shown best in the change from the previous test. The percentages of the changes which are two or more times the average for the control tests are for the different observers:

(2) Observer E, 67 per cent, (4) Observer G, 83 per cent,
(1) Observer F, 29 per cent, (3) Observer H, 83 per cent,
(5) Observer J, 0 per cent.

These figures eliminate from consideration the general tendency to decrease rate and consider only the amount of change from test to test. Observer J, again, is shown to change by an amount equal to that for the control tests. But the observers now change in amount from test to test in relation to the order of increased variability in the control tests (as shown by the figures in parenthesis), which indicates that the efficiency of each individual observer in the experimental test corresponded to his efficiency in the control tests, while he might be sliding down the rate scale for Test II.

Table VII averages by tests and by observers the ratios of change given in the table in the Appendix. All averages of ratios for observers and tests show changes which are more than double these changes in the control tests, with the exception of those for Observer J. It is evident (excepting in the case of Observer J) that the effect of the experimental situation was to increase greatly the change in rate of performance either in relation to the starting rate or in relation to the rate of the previous test.

The evidence in Table VII significant to the causes of rate change lies in the size of the S.D.s of the distributions of ratios both for observers and for tests. If the ratios were similar for tests, i.e. the S.D.s of the distributions of observers' ratios for any one test were low, it might be interpreted that the same stimulating situation was operating as the causative factor in all observers for each individual test. If the ratios were similar for observers, i.e. the S.D.s of the distributions of test ratios for any one observer were low, it might be interpreted that the original general intent to work without change in rate in all tests was operating as the causative factor. The irregularity of the changes for tests and for observers does not indicate either of these conclusions. Evidently the causative factor was individual as relates different tests and different observers, which supports the conclusion, drawn from the introspective study of intent, that the instructions of each experimental test engendered additional specific intent of an individual nature for test and observer as the essential cause of the changes in performance.

Table VII. *Mean ratio of change by tests for five observers (E–J incl.) and by observers for all tests*

Tests	Instructions	I Mean ratio (change from Test II over av. absolute change from Test II in control tests)		II Mean ratio (change from previous test over av. absolute change from previous test in control tests)	
		Mean	S.D.	Mean	S.D.
VI	Warning	2·1	1·4	2·2	1·8
VII	A	2·9	1·9	1·9	0·6
VIII	B	1·8	0·1	1·6	0·1
IX	C	4·8	0·5	4·4	2·8
X	D	2·8	1·8	1·9	2·1
XI	E	5·1	2·5	3·4	2·7
XII	Recapture original rate	3·7	2·3	3·1	2·4
	Mean	3·3	1·5	2·6	1·8
Observers					
E	—	5·1	0·9	3·1	1·8
F	—	4·3	1·3	1·3	0·9
G	—	1·8	1·2	3·1	2·6
H	—	3·7	1·8	3·9	2·2
J	—	0·65	0·5	1·0	0·4
	Mean	3·1	1·1	2·5	1·6

In reviewing the abstracted introspective reports one might conclude that *general* increase of conscious activity was the cause of the greater change in rate in the experimental tests; but it is evident that this conscious activity was largely *intentional* conscious activity. Specific additional intents coming out of the confusion or disruption of the established set in the control tests would appear to have been the cause of increased variability in performance. The relationships between specific additional intent and changes in performance cannot be exactly determined, because of the descriptive nature of intent. But in at least 85 per cent of the tests of these five observers that performance followed specific intent in each individual test (see Appendix). Specific intent develops in relation to some general background of mental set, as it has been established in previous tests, and the effect is an integrated effect. But it seems justifiable to conclude that significant changes of performance are connected with definite change of intent, although there is some amount of variability without this.

VIII. SUMMARY AND CONCLUSIONS

This investigation studied the effect of conscious intent upon performance and in particular the causative relation of specific intents to changes in performance.

(a) *Survey of results of previous experiments*

In earlier studies by the author (2, 3) it was found that any change in performance was correlated more exactly with changes in intent than with changes in environmental stimulation, and the general conclusion was drawn that environmental changes, whether verbal or concrete, must affect intent before influencing accomplishment. It was found that there was no significant change in performance without a co-existing development of intent.

It was indicated in one investigation (2) that concrete and verbal stimulation may be equally effective in changing performance, but their effectiveness is always through the channel of conscious intent. The effect of accented auditory timing was studied in detail (3) and rate of auditory timing was found to have no direct effect, either general or specific, upon rate of work. It seemed that environmental stimuli are effective in changing performance only when they are accepted, or chosen, in the development of intent. Intent is highly individual in development, and here is an explanation of variability in distraction experiments with tasks well within the capacity of the subjects. Specific intents are in continual development through an increased awareness of environmental stimulation, and they re-define the general set in relation to the task.

(b) *Summary of results in this experiment*

The conclusions of earlier investigations formed the basis for the present experiment, which stabilized the general intent (to do the task at a comfortable rate without change) and studied the effect of specific additional intents in relation to performance through introspective reports of conscious activity, during a series of experimental tests, in which a variety of contradictory and confusing instructions were administered. The general intent was established in an introductory test. A series of control tests, where the general intent was maintained, were compared in respect of variability with the experimental tests. The whole experiment consisted of twelve tests, each requiring about a minute of working time, which were performed during one period of work averaging about 1½ hours. Six trained observers were used in the experiment.

In the initial test the observers were seeking cues to maintain rate in accordance with the instructions not to change rate after the first test. Different cues to maintain rate were used by the different observers, and they took the form of visual sensitivity, of movement, and of verbal-motor and arm kinaesthetic sensitivity. These same cues for controlling and checking rate were used throughout the control tests. Variability was low in the control tests with an average S.D. for all observers in these tests of 2·35 per cent at an average rate of 95·2 problems per minute. This is well within a standard of variability derived from a previous study of variability with uniform intent (4). It was shown by the control tests that consistently focal intent with definite cues for maintaining rate is a condition of low variability.

One observer (Observer A) adopted an intent to follow the instructions of the experimental tests and the ratio of variability in these tests to control tests was 12·21, which indicates that variability increased 12·21 times in these tests. His performance correlated exactly with reported intent in each test, which is additional evidence for the early conclusion (2, 3) that significant changes in performance are always intended.

This investigation was concerned primarily with causes of slight changes in performance, and whether or not environmental changes cause these slight changes in performance without any change in intent. Five observers maintained their original intent throughout the experimental tests, i.e. to work at a comfortable rate without change in rate after the first test. In the experimental tests their introspections showed increased conscious activity, increased awareness of rate cues, and additional intent, related to the new instructions, to keep effective the original intent.

Performance decreased considerably in rate in the experimental tests. The average of the five observers in the control tests was 100·39 per cent of problems per minute and the average in the experimental tests was 91·64 per cent of problems per minute. Individual variability for the experimental tests was increased, with an average ratio of experimental tests to control tests of 1·79. All of the observers increased variability and three of the five more than doubled their variability.

When an analysis of the data is made for causes of increased variability it appears that specific additional intent, which is individual for test and observer, was the causative factor. As the experimental instructions were administered, each observer intended to work at this original rate without change, but he intended to do so by counteracting what effect he thought the new situation might have upon him. As the new instructions were more frequently to increase speed, he usually intended to restrain speed, thus the tendency was for average rate of performance to decrease. In so far as it is possible to judge the relation of intent to performance the changes in performance were correlated with changes in intent. In eighty-five per cent of the tests performance followed these specific additional intents, which justifies the conclusion that any significant change in performance is intended.

These results are for Observers E, F, G and H, with Observer J an exception. Observer J is the 'fly in the ointment' in this experiment, as far as average results are concerned. But his results are most valuable in understanding the effect of intent upon performance. He started out with high variability in the control tests and showed only slightly higher variability in the experimental tests. Also his rate did not decrease in the experimental tests. According to his introspections he never achieved any definiteness of intent. He just intended to let himself go. Hence, the experimental instructions did not have the effect upon him that they did upon the other observers who used detailed cues in maintaining their rate. When these cues were disturbed by the new instruction situations these observers became more variable, but even then variability was not as great as it was throughout for Observer J.

(c) *Conclusions*

Important conclusions developing out of the present experiment follow:

(1) *Intent and rate of performance.* Intent determines rate of performance with a high degree of exactness.

(2) *Environmental changes.* Environmental changes, in the form of new instructions, (1) may affect variability of performance through intentional acceptance or rejection of these changes as conscious stimuli, or (2) may affect variability through the development of various specific intents to control performance in the new situation.

(3) *Specific intents.* Various specific intents are individually developed to counteract possible effects of changing environmental situations upon any performance which is guided by uniform intent. Changes in performance are closely related to these specific intents, which appear to be the essential causative factors, rather than the changes in the environment or increased general awareness of the situation.

A statement should be added to these conclusions upon the use of conscious cues in rate guidance. Intentionally used cues in rate guidance vary with individual observers. Variability in performance seems to be related to the degree of focus in awareness of intentionally used cues. This statement needs further verification, which is not possible with the data of this experiment.

Less is implied concerning the relative efficiency of different cues for rate guidance. It is possible however that certain cues are more efficient than others in any given situation. A previous experiment indicated that all observers employed verbalization to some degree, and it is suggested here that this cue increases in overt expression as the environmental situation increases in complexity (see also Morgan (5)). Further, it is suggested that environmental changes, in the form of new instructions, have a third effect (not mentioned in the second conclusion above) of disturbing the focus of intentionally used cues and may thereby affect variability of performance. The cognitive side of intent is an important and inadequately explored activity.

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APPENDIX

Comparison between additional specific intent with new instructions and the change in performance from Test II of control tests (and ratio of this change to the absolute average change from Test II for control tests of this observer) and the change from previous test (and ratio of this change to the absolute average change from previous test for the control tests of this observer) all in terms of percentage of Test II

			Change from Test II		Change from previous test	
Test	Instructions	Additional specific intents	Amount	Ratio to control	Amount	Ratio to control
		Observer E				
VI	Warning	Loss of definiteness and to restrain speed (emotional excitement)	– 13·07	4·3	– 8·49	5·6
VII	A	Restrain speed and not follow new instructions	– 16·70	5·6	– 3·63	2·4
VIII	B	—	—	—	—	—
IX	C	Obliterate bells and not write with them (emotional excitement)	– 14·12	4·7	+ 2·58	1·7
X	D	Oppose tempting instructions and work at normal speed	– 13·45	4·5	+ 0·67	0·4
XI	E	Compensate for suggestions by restraining speed	– 20·52	6·9	– 7·07	4·7
XII	Recapture original rate	Write to rhythm, ‘now, now, now,’ etc.	– 13·93	4·7	+ 6·59	4·3
		Observer F				
VI	Warning	No additional intent (increased awareness about implications of instructions)	– 6·04	2·3	– 4·70	2·3
VII	A	Not to follow instructions and possibly restraint	– 8·38	3·1	– 2·34	1·3
VIII	B	Not to follow instructions (awareness of rate of task)	– 8·49	3·2	– 0·11	0·05
IX	C	Not to work in rhythm (increased awareness of rate)	– 13·30	5·0	– 4·81	2·4
X	D	Not to follow instructions	– 13·52	5·0	– 0·22	0·1
XI	E	Not to follow instructions and possibly restraint	– 16·54	6·2	– 3·02	1·5
XII	Recapture original rate	No additional intent	– 14·08	5·3	+ 2·46	1·2
		Observer G				
VI	Warning	Watchfulness (greatly increased awareness of task)	+ 4·10	1·3	– 1·45	0·8
VII	A	Vague tendency to follow new instructions (questioning correct procedure)	– 0·71	0·2	– 4·81	2·6
VIII	B	Vague tendency to follow new instructions (questioning correct procedure)	+ 3·30	1·0	+ 4·01	2·2
IX	C	Emphatically not to follow instructions and restrain rate	– 13·02	4·0	– 16·32	8·8
X	D	Not to follow instructions but go at old rate	– 8·68	2·7	+ 4·34	2·3
XI	E	—	—	—	—	—
XII	Recapture original rate	Let work go at old rate (day-dreaming)	– 5·06	1·6	+ 3·62	2·0

Test	Instructions	Additional specific intents	Change from Test II		Change from previous test	
			Amount	Ratio to control	Amount	Ratio to control
		Observer H				
VI	Warning	Expectancy (increased awareness of implications of instructions)	− 5·61	2·6	− 1·32	0·9
VII	A	Not to compensate by going slower (increased awareness of situation)	− 8·71	4·0	− 3·10	2·1
VIII	B	To increase speed to original rate	− 4·89	2·2	+ 3·82	2·5
IX	C	Increase verbalization and slight compensation (increased awareness of task)	−11·93	5·4	− 7·04	4·7
X	D	Not to follow instructions (increased awareness of situation)	− 3·11	1·4	+ 8·82	5·8
XI	E	To be slightly slower at points (increased awareness of task)	−14·07	6·5	−10·96	7·2
XII	Recapture original rate	—	—	—	—	—
		Observer J				
VI	Warning	Expectancy	+ 0·10	0·02	−11·74	1·2
VII	A	Must not speed-up	−10·93	1·5	−11·03	1·1
VIII	B	Not to go faster or slower	+ 5·50	0·8	+16·43	1·7
IX	C	—	—	—	—	—
X	D	Not to follow instructions (increased awareness of situation)	− 2·43	0·3	− 7·93	8·0
XI	E	Not to pay any attention and let myself go (emotional tension)	− 5·62	0·8	− 3·19	0·3
XII	Recapture original rate	Let speed go	+ 3·49	0·5	+ 9·11	0·9

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NON-HOMOGENEOUS SENSATIONS: A NEW THEORY OF SENSATION DISCRIMINATION

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I. *The problem* (pp. 394–395).
II. *The non-homogeneous sensation theory* (pp. 395–396).
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I. THE PROBLEM

WHEN a stimulus acts on one of our sense organs, we are conscious of a sensation of a magnitude which depends on the energy in the stimulus; in order that a second stimulus should produce a sensation which we can just distinguish as being greater than the first, the energy in the second stimulus has to be greater than the first by a finite increment. Thus if we listen to two musical notes in succession, we can detect when one is just louder than another when there is a certain finite difference in the energy falling on the ear in the two cases. The same applies to the detection of a difference in brightness in two patches of light, viewed either side by side or in succession, of the difference in temperature of two bowls of water, of differences of taste and smell, weight, colour and so on. Further, it is found that if I is the intensity of the stimulus and ΔI is the increase necessary to produce the just noticeable difference in sensation, then $\Delta I/I$ is constant to a fair approximation for a mean range of intensities, neither very high nor very low.

This law, due to Weber, is not the immediate problem we wish to investigate. The more fundamental problem is to find a cause for the limitation of our power of discrimination. Why, in fact, is there a finite limit at all? The magnitude of a sensation varies continuously and it is not immediately apparent where the limitation in sensitivity arises.

The fact that nerve currents are transmitted as a series of discrete impulses does not imply a finite limit, since the rate at which the impulses are transmitted can vary continuously. An alternative theory suggests that each step in discrimination occurs as the threshold of a nerve ending, e.g. a rod or cone in the retina, is passed. This is unsatisfactory in that it takes no account of the continuous increase in the response along those nerve fibres the threshold of which has already been reached and further, it requires a tremendous range of threshold values to account for the existence of discrimination steps at both very high and very low intensities.

II. THE NON-HOMOGENEOUS SENSATION THEORY

The explanation for a finite limit put forward here is based on the hypothesis that when a stimulus acts on one of our sense organs, we are conscious of an apparently homogeneous sensation which is in fact composed of a large number of sensations differing slightly from one another both in quality and magnitude.

Consider the case of two patches of light that are being compared. According to the hypothesis, each patch is composed of a large number of elements all of slightly different apparent brightnesses. Suppose that in each light patch there are 1000 elements and that their brightness varies in one field from 95 to 105, expressed in some arbitrary unit. It would evidently be very difficult to observe any difference in brightness if, in the second field, the range of brightness was 95·5–105·5 or even 96–106. Possibly if the brightness were increased to give a range of 97–107 or 98–108, a change could be detected. It is evident that the difference between the average intensities of the two fields when one is just noticeably brighter than the other, will be very closely related to the spread of 10 units assumed to exist in the brightness of the individual elements.

Although the theory postulates a variation of brightness over the patch, this variation may not be consciously observed. The individual elements, which in the case of the eye would presumably correspond to the individual nerve endings in the retina, cannot be distinguished, since they are at or below the resolvable limit of the eye. Further, the variation in brightness from one element to another is not likely to be large. Although not consciously observed, a spreading of the type suggested would affect the accuracy with which the brain could estimate the average magnitude of the sensation and would thus control the size of the discrimination step.

A possible extension of the theory would allow for the variation to occur in the individual fibres. That is to say, instead of the spreading arising solely from the different sensitivities of the various nerve paths, each nerve fibre itself might conceivably vary in sensitivity from moment to moment. This would have an exactly similar effect in limiting the discriminating power.

Turning from the eye to the other sense organs; in the case of hearing, although with a single note there is no spatial distribution of the sensation, yet its magnitude is the resultant of a number of superimposed responses from different nerve paths, and any variation in these responses will be sufficient to impose a discrimination limit. In testing the temperature of a bowl of water, if different areas of the finger or hand immersed in the water register different sensations of heat, a finite limit will be implied. Similarly in taste and smell, colour, weight and so forth.

The theory may be summarized in the statement that any sensation is the mean of a number of independent sensations, that these elementary sensations are assumed to vary in intensity from one to another and that this variation will limit the accuracy to which the magnitude of the mean intensity can be estimated and will thus impose a finite discrimination limit.

III. PHYSIOLOGICAL SUPPORT

It is not the purpose of this paper to discuss in any detail the possible physiological origin of the variations we have postulated. We are only concerned with demonstrating that a finite discrimination limit is necessarily implied if such variations do exist. But in order that the theory should avoid being too vague, three possible causes of the spreading may be mentioned: variations in sensitivity of the nerve endings, variations in the efficiency of the nerve paths and variations in the brain cells. If there were to be no spreading, each of these links would have to be identical from nerve to nerve. Such uniformity is incredible.

As a concrete illustration of the type of variation in mind, some work reported recently by Graham & Hartline (1) may be quoted. In this work, the luminosity curves for different cells in the eye of the limulus were recorded, and small but significant differences were found from cell to cell.

IV. EXPERIMENTAL SUPPORT

A simple experiment can be carried out to demonstrate that non-uniformity of brightness of a patch of light will affect the size of the just noticeable intensity difference. The author's colorimeter, arranged for intensity discrimination as described in (2), was used with a filter of irregular density inserted in the beam. The 2° field thus no longer appeared uniform, but possessed slight variations in intensity over its area. The total variation was limited to about 20 per cent of the mean value. When ΔI was determined under these conditions, a considerable increase was found compared with the value obtained with a uniform field. Similarly, the accuracy with which the two patches could be matched was reduced. The size of the just noticeable difference and the range over which the two fields may appear matched are, of course, intimately related and the determination of the one is equivalent to the determination of the other. In the present observation, the matching method was preferred, as any tendency for a knowledge of the problem to affect the judgment of discrimination was more easily avoided.

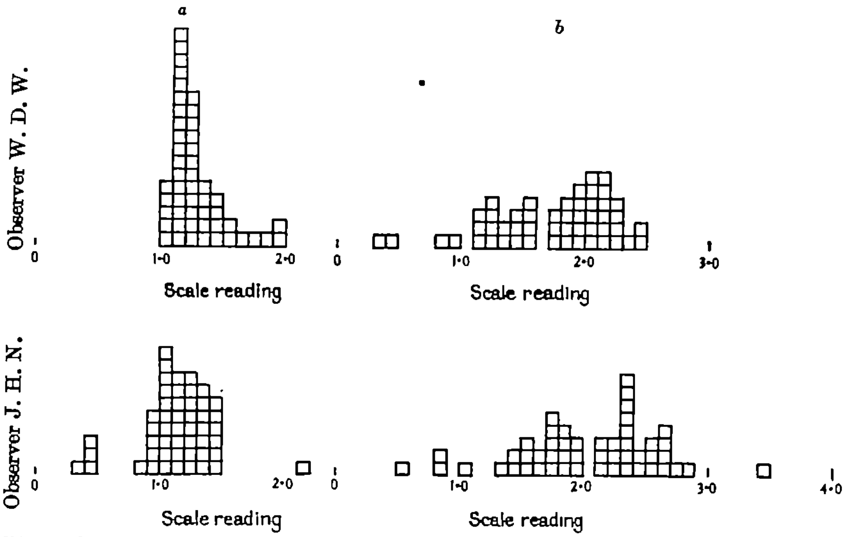

Fig. 1. Diagram showing the spreading of observations obtained when making intensity matches between two fields, (*a*) when the fields are uniform, and (*b*) when the fields are non-uniform. (Each square represents one observation and is plotted at the scale reading obtained.)

The distribution of 50 matches (*a*) when the field is uniform and (*b*) when the field is uneven, obtained for two observers, is shown in Fig. 1. It will be seen that the spreading of the observations is much greater in the case of the uneven field.

This experiment does not necessarily verify the theory, but it does prove that any variation in brightness that may exist will affect the size of the discrimination step. It may therefore be regarded as valuable circumstantial evidence in the theory's favour.

V. Conclusions

Any conclusive verification of the theory will be difficult. But it seems certain that, once the existence of the hypothetical variations is granted, its effect on the discrimination function must also be granted. It is not possible to say at this stage whether the variations would exert the main control on the size of step, but it is a reasonable assumption to make.

It is evident that the significance of Weber's law, the constancy of $\Delta I/I$, would be materially affected if non-homogeneity of the sensation governs the magnitude of ΔI. The law would automatically follow if the spreading were a constant of the physiological mechanism.

Another attractive feature of the theory is that it does explain the uncertainty with which discrimination limits are observed. No state exists at which the difference is definitely below the threshold at one moment, but where a very small increase immediately brings the difference above the threshold. There is always a region in which it is extremely difficult to decide whether or not a difference can be detected. This is exactly the experience that would be anticipated on the assumption of a non-homogeneous sensation.

If it were possible to control the extent of the spreading, it ought to be possible to vary the size of ΔI. A reduction in the uniformity of response might thus be the cause of diminished sensitivity when the observer is tired or unwell. The theory may also provide the explanation of such phenomena as the effect of field size on visual discrimination.

It can at least be stated with certainty that when discrimination experiments are being conducted, physical uniformity of the stimulus is very important.

VI. Summary

The fundamental problem in sensation discrimination is to find the cause of the finite limit to our discriminating power. A new theory is proposed, based on the assumption that no sensation that we perceive is in reality a homogeneous sensation. The change in stimulus necessary to produce a just noticeable intensity difference is on this theory governed by the variation among the individual elements of the sensation.

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THE NATURE OF 'DIFFICULTY'; WITH REFERENCE TO A STUDY OF 'WHOLE-PART' LEARNING

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I. *Abstract of experiment on whole-part learning* (p. 399).
II. *The nature of difficulty* (pp. 399–403).
  (1) *Procedure* (p. 400).
  (2) *Results* (pp. 400–402).
  (3) *Discussion* (pp. 402–403).

I. Abstract of experiment on whole-part learning

This study[1] was an attempt to find the relative efficacy of the whole and part methods of learning by using material having various degrees of difficulty for the subjects. In it children of three levels in the school were subjects, and the material consisted of four poems of equal length and style but of increasingly difficult 'thought content'. The degree of difficulty of each was taken to be determined by the number of words learned by all the subjects in equal learning periods. Groups equated in mental age at each of the levels learned the poems by either the whole or the part method. Recall tests were given.

It was found that for each grade one poem gave maximum superiority for the whole method, and that with easier or more difficult poems this superiority decreased. The maximum difference between the methods occurred on succeedingly difficult poems for succeedingly higher grades. Thus it was suggested that the whole method of learning is most effective when the material has a certain degree of difficulty for the mental ability of the child, and that as the material becomes either easier or harder than this, the difference between the methods becomes increasingly less.

II. The nature of difficulty

While it may be quite legitimate to score difficulty in terms of the amount learned under equal learning conditions, this gives no clue as to what is the nature of difficulty, and what is meant by saying the thought content of some of these poems is 'harder' for the subjects, than that of others. What happens to the material a subject learns badly, and which we say, 'he finds difficult', is suggested by the results of a recall test given some seven weeks after the first of the poems had been learned.

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[1] Northway, M. L. (1934), "Difficulty of the task and ability of the subject as factors in whole-part learning". *Unpublished thesis*, Univ. of Toronto library.

(1) *Procedure*

The following are the poems which the children learned in succeeding weeks:

1

My little dog, I call him Jock,
Goes out for walks with me;
I take him all around the block,
He's good as he can be.
He stays beside me and we talk,
He answers when I call,
But when a squirrel runs down the walk
I can't find him at all.

2

The moon is up; the stars are bright,
The wind is fresh and free;
We're out to seek for gold to-night
Across a silver sea.
The world was growing grey and old;
Break out the sails again!
We're out to seek a realm of gold
Beyond the Spanish main.

3

Without a wooden fence or hedge,
Half wild, yet wholly tame,
The wise turf cloaks the white cliff edge
As when the Romans came.
What sign of those that fought and died
At shift of sword and sword?
The barrow and the camp abide,
The sunlight and the sward.

4

I leant upon a coppice gate
When frost was spectre grey;
And winter's dregs made desolate
The weak'ning eye of day.
The tangled vine stems scored the sky
Like strings from broken lyres,
And all mankind that haunted nigh
Had sought their household fires.

Seven weeks after the first of these had been learned, and 4 weeks after the last, the children in the middle group (av. mental age 140 months) were asked to write "what they remembered of each poem, or what story it told". From this data lists of the items, phrases, and ideas which differed from those of the original were made, and from these arises a suggestion about nature of difficulty, and what the term implies.

(2) *Results*

Table I gives the number of words learned and reproduced immediately for each poem. This was the objective criterion of the degree of difficulty.

Table I

Poem	1	2	3	4
Av. words	40	38	25	18

With subjects of this middle group the whole method was most superior with poem 2.

Under each poem examples of the type of recall obtained will be given.

Poem 1.

Except for minor changes such as designating the subject as 'a boy' or 'a girl' and the object of the dog's pursuit as 'a cat' or 'another dog', the story of the poem remained very close to the original.

Poem 2.

The number after the example shows the number of words the subject gave in his original recall. These are answers to "what was the poem about?"

(1) Pirates. (36)
(2) A nice clear eve and sort of cold out. (42)
(3) Nature. (38)
(4) A group of people getting ready for a fishing trip. (43)
(5) Boy or girl going to bed and the stars and moon are shining and the child loves to see them. (44)
(6) About snow and some man riding in it. (48)
(7) The moon is up, the stars are out, come out and play with me. (42)
(8) The night has come and the little girl is sleeping and dreaming how she is going to get gold across the Spanish Sea. (25)

These show a decided trend away from the original material, and what is given includes (*a*) items from the child's general experience (the fishing trip, and the dream), (*b*) items from other poems in the series (snow), (*c*) items from other poems ("Come out and play with me"), and a conventional answer (about nature).

Poem 3.

It's about:

(1) If you haven't got a fence you can climb the trees and play in the sunshine. (36)
(2) A man who has no home nor nothing to his name. (16)
(3) A man describing the chalk cliffs of Devon. (24)
(4) A boy sitting on a cold chair, looking out the window, at a fence, his mother calls him to go to the store so he goes. (26)
(5) About flowers growing in a garden. (8)
(6) Birds and a boy sitting at a lake. (26)

These show further change from the original and more diversity in the answers given. As in the previous poem, items (*a*) from the child's experience (lake, climb, going to store) and (*b*) from other poems—as well as (*c*) merely details (cliff)—are recalled.

Poem 4.

It's about:

(1) He lent a copper gate to somebody. (25)
(2) A woman who walked a long way up a hill. She rested on a gate and a car came and hit her. (10)
(3) A little girl hanging on a gate, looking out to the lake and listening to the birds. (21)
(4) About a girl leaning against the gate. Her boy friend passed and did not talk to her, so she did not go to the seashore that day. (8)
(5) "I leant upon a coppice gate,
I heard a shoemaker singing." (33)

These show the same points as the former poems but contain much more invention, and show more diversity among the answers. The diversity cannot be shown in these few examples, and the reader will have to accept by faith that each succeeding poem shows not only more invention but also more individuality in the types of inventions which are given. It should be noted that no answers entirely true to the original are given.

(3) *Discussion*

From these results the following points are suggested:

(1) As recall becomes less accurate, that is, where less was originally learned, or where more has been forgotten, invention occurs to a greater degree. Thus the stories given for poem 4 have more new phrases and ideas than those for poem 1. This is in accordance with Bartlett's[1] suggestion that memory, which is not merely 'rote', is a construction rather than a reproduction.

(2) A suggestion about 'difficulty' is indicated from these results. The poems were learned by mere repetition, no attempt was made to clarify the 'meaning', yet the children interpreted more or less correctly the main substance of poems 1 and 2, and wandered farther from the original in poems 3 and 4. It might be suggested that poems 1 and 2 were meaningful to the children and the later ones beyond their comprehension. This free reproduction method shows what actually occurs to the material which we have labelled 'difficult'. It is simply transformed into something which is meaningful and satisfactory to the child, and he is unaware how much his recall diverges from the original.

To generalize: The difficulty of material depends upon how foreign it is to the subject's psychological field, and how much he has to change it in order to absorb it. The amount he omits is probably not as representative of the meaningfulness the material has for him, as is the amount he invents or constructs which has no apparent relation to the original. It would seem then that what has traditionally been called memory is one particular form of the creative process, and that when a subject is asked to recall material, if it is not too difficult for him his creation will correspond to that objectively given material, but if it is too difficult, or too 'foreign', his creation will differ from the original often to such an extent that no apparent relationship appears.

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[1] Bartlett, F. C. (1932), *Remembering*, Chap. x. University Press, Cambridge.

If difficulty is a function of the amount that material has to be re-formed in order to become meaningful, the reason that whole learning is most superior for certain material becomes clearer. A certain poem is of such difficulty that it is meaningful to the subject, and becomes part of his psychological field when it is presented to him as a whole; when, however, it is broken into parts, these parts as such are not absorbed by the subject, but have to be knit either into the whole which corresponds to the objective whole given by the poem, or into some other whole satisfactory to the subject. That is, the parts have to be transformed, while the whole is absorbed in its own form. With more 'difficult' material both the whole and part presentation have to be transformed by the subject, and with much easier material, both presentations can be absorbed in their own right.

This idea of 'difficulty' as foreignness or distance from the subject's psychological field may throw some light on what, in the educational world, are called children's mistakes or 'wrong answers', and also on that rather specialized type of invention known as 'children's lying'. It also provides a basis for understanding invention as it occurs in day-dreams.

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VISUAL AND TACTUAL 'JUDGEMENT' AS ILLUSTRATED IN A PRACTICAL EXPERIMENT

By HENRY BINNS

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I. *Introduction* (p. 404).
II. *The plan of the experiments* (p. 405).
III. *The 'matching' of visual and tactual judgements* (pp. 405–406).
IV. *Errors of visual judgement compared with errors of tactual judgement* (pp. 406–408).
V. *Some individual results* (pp. 408–409).
VI. *Summary* (p. 409).
References (pp. 409–410).

I. Introduction

In the present paper I propose to consider briefly three points which have arisen from a long series of investigations of a technical nature and with a practical aim. About eighteen years ago I became interested in the bases of visual and tactual judgements of the fineness, softness and value of wool textiles. The present discussion is concerned with results of experiments using wool-tops only as material. These consist of a mass of fibres, the whole about an inch thick and the individual fibres lying parallel one to another in a rope-like, untwisted 'sliver'. My problems were to find out how the average results of a number of judgements of fineness and softness of the fibres in wool-tops are related. Which, if either, of the two types of judgement is the more dependent upon specialized training? Are the most reliable judgements arrived at by either type separately, or by both in combination? The detailed results of my many experiments have already been published in a series of papers (1–8) and need not be repeated here. But the whole investigation raised a number of points of general interest to psychologists, and perhaps contributed something towards their solution. To three of these only I now propose to direct attention.

II. THE PLAN OF THE EXPERIMENTS

The general plan of the experiments was as follows:

Wool fibres were 'drawn' out from a top and placed on a black velvet-covered board in such a manner as adequately to display their characteristics to vision. This process of 'drawing' requires considerable skill, and it was always carried out by myself with the greatest possible care. New 'drawings' were made on each occasion of testing. Six samples were used. They comprised the International Standard accepted by the British Wool Federation and the U.S.A. Board of Agriculture. They were graded by standard numbers as representing six typical Bradford tops which could be arranged in an increasing order of fineness or of softness.

For tactual judgements 12 in. of each top were employed. A given sample was held gently in the left hand of the observer, who then explored delicately the mass of fibres between the fingers and thumb of the right hand, at the same time drawing the sample across the palm.

No observer using a visual method was allowed on that occasion to touch the samples, and no subject employing a tactual judgement was permitted to see the sample. A subject of a visual experiment was, however, able to move the boards, without touching the fibres, if he desired to do so. The instructions were simply that samples must be graded either (1) in order of fineness by sight, or (2) in order of softness by touch. In any given set of experiments each sight and touch sample was presented in haphazard order five times.

III. THE 'MATCHING' OF VISUAL AND TACTUAL JUDGEMENTS

The results of experiments carried out on a large number of individuals belonging to a diversity of different occupational, age, and 'interest' groups show an astonishing agreement of grading as between visual and tactual methods. In only about 12 per cent of the instances was there any significant difference in grading when visual judgements were compared with tactual, and both were in close agreement with the standard arrangement of samples in the vast majority of cases. It is, of course, obvious that this may mean that the steps from sample to sample were definitely supraliminal for both vision and touch, but this does nothing to destroy the interest and importance of the agreement. For the sensory patterns underlying each type of judgement must have been markedly different. The criterion for visual gradings was 'fineness'; for touch it was 'softness'. Visual judgements were based on the apparent average diameter of the fibres. Tactual judgements, on the other hand, were based on the relative softness or evenness of groups of fibres aided by the impressions set up by the fine saw-like serrations of wool as they came into contact with the surface of the skin.

Several years ago Sir Henry Head pointed out that 'matching' may be regarded as a relatively low-level process, uncomplicated by any necessity for symbolic formulation (9). It is true that his cases were somewhat different from mine, since he was interested in showing that different sensory patterns could be identified as belonging to the same object, whereas I have shown that when sensory patterns having a different mode are utilized as a basis for judgements concerning specific qualities of the groups of stimuli which arouse them, these judgements may show a high degree of agreement. It seems highly desirable that a thorough investigation should be planned into the efficiency, limits, range and precise breaking-down points of matching, where reactions having a different sensory or general experimental basis are concerned. And it seems to me that both the materials and the methods of my investigation could be made a good starting-place for such an investigation.

IV. ERRORS OF VISUAL JUDGEMENT COMPARED WITH ERRORS OF TACTUAL JUDGEMENT

On the whole remarkably few errors of placing were committed both with vision and with touch. Taking the whole series the average number of misplacements for any given set were twelve by sight and eleven on a tactual basis, and many of these were in fact of slight significance. Moreover the sight and touch judgements intercorrelated very highly and both agreed almost perfectly with the grading on International Standard.

Since the standard samples were selected by the most capable men in the wool world, both American and English, and after the most impartial and exhaustive judgements had been made by them, the data show beyond doubt the efficiency of the sight and touch gradings.

However, if the errors of placing—i.e. transpositions within the judged series, as compared with the standard series—of visual and tactual, or handle, judgements are considered for each group of subjects, some interesting results emerge, as may be seen from the Table I.

It will be noticed that only in one group, that of the topmakers, have the visual judgements any marked advantage. They show a superiority also in the case of the schoolboys, and a slight superiority in that of the technical students. In all other cases the advantage, fairly marked in the instance of the arts and crafts students, considerable in that of the schoolgirls, but slight in the other groups, is on the side of the touch, or

Table I

Type of group	Ratio of visual to tactual errors of placing	
	Visual	Tactual
Topmakers	1	6
Trade (various)	2·75	1
Technical students	1	1·2
Art and crafts' students	2	1
Other trades and professions	1·5	1
Professional women	1·2	1
Schoolgirls (age 11 +)	3·5	1
Schoolboys (age 11 +)	1	2

handle, judgements. It is naturally not easy to interpret these results. The superiority of one or the other method might be due to some abnormality of functioning of the special sense apparatus in the individual members of the groups concerned. But the distribution of the errors makes this look unlikely. Individuals who showed a very pronounced discrepancy in the one type of judgement as compared with the other have not been included in Table I, and will be considered in the next section. It is tempting to suggest that visual judgements in this case are more dependent upon specialized training than are handle judgements.

If this were true one or two very interesting points would emerge. The technique of the handle experiments means that the tactual judgements are being based upon a complex of simultaneously present cutaneous reactions. There is active exploration with the finger-tips, where, as is well known, the threshold of discrimination is normally very low. But there is also a massed result of all the cutaneous properties which can be appreciated in this case as the palms are closed and the sample drawn slowly through them. These are based upon the chemical properties of the structure and elasticity of the wool which can be expressed in terms of softness owing to the apparent regularity or irregularity of surface contour. All these qualities here function together and are evaluated as a whole by the observer. A particularly sensitive or well-trained person might, perhaps, be able to isolate the various factors involved, or it may be that this is another of the many cases in which the observer must base his judgement upon functional qualities the nature of which he is, and possibly must remain, ignorant. Meanwhile it is probably true that tactual discrimination is increased by the simultaneous functioning of a number of variable conditions, and so becomes finer than it could be were any of the variables presented alone. Whether these suggestions can be fully confirmed or not must await additional investigation for which, once again, both the material and the methods of my experiments seem to be apt.

V. SOME INDIVIDUAL RESULTS

Certain individual results stood well away from the general trend which, as already indicated, was towards a remarkable amount of agreement of sight with touch judgements. For example, woolmen with many years of varied experience behind them have expressed themselves as 'staggered' by the results obtained from three of the schoolgirls. Two of these made no errors whatever in grading by touch, and the other had two errors only with touch although she made no less than thirty-six mistakes with vision. Further, a woman who had formerly been a school-teacher had no errors with touch, but eight with sight. Three art teachers made only two mistakes each on a tactual basis of grading. Not one of these people had been given any prior specialized training; they all worked rapidly and confidently, and in each case the handle judgement was accurate beyond anything that could possibly have been expected. Another man who made forty-four errors of placing with vision was mistaken only twelve times with touch. In my opinion these samples and tests constitute a reliable measure for tactual sense.

On the other hand a topmaker who made no misplacements with sight made ten with the handle method; a spinner, also making no errors with vision made sixteen with touch, and one schoolboy who made fifty-six errors when he was grading by handle made only ten when he was allowed to employ vision.

Undoubtedly some of these discrepancies must be set down to deficiencies in the particular special sense mechanisms involved, and these, together with their effects, require a more serious study. But it is utterly unlikely that they should be the only, or even, in all probability, the main sources of the differences. It is, in fact, strongly suggested that, apart from differences of absolute or relative visual and cutaneous sensitivity, there are other factors which assist or obstruct the kind of sensory judgement which was investigated in these experiments. If this be the case, problems both of theoretical and of practical importance are involved. In theory we must know more definitely what these 'other factors' are, and how they operate. For practical purposes, if any person is to be asked frequently to pass judgements of the kind indicated it is highly important, alike for his selection and for his training, that he and others should know by what method he can achieve the best results with the least difficulty.

Here obviously is one of the fields for the application of vocational guidance. But it is necessary to proceed with very great caution in this direction. The usual thing is for an observer to show a considerable fluctuation of efficiency in grading from time to time, even when he may be using the same basis of judgement on different occasions. For instance, to quote an actual case; an observer whose average number of errors of grading with the visual method was fifteen, sometimes made six mistakes only and at others mounted to twenty-two. The mean errors of the same observer with the tactual method were ten, and his variations were from six to fourteen mistakes. Careful investigation is needed, in this matter as in most of the others where vocational guidance is alleged to be based largely on a test procedure, in order to discover what fluctuations of grading on visual as against tactual methods may be regarded as of genuine significance.

VI. SUMMARY

The paper is based upon many years' research into the relation of visual and tactual judgements for the grading of wool samples. It is shown:

(1) That visual patterns for 'fineness' of wools and tactual patterns for their 'softness'—taking the average of a large number of observations —'match', or coincide, to a high degree.

(2) That it may be the case that the visual judgement is the more technical and trained; the tactual, or handle judgement is the more native and immediate.

(3) That marked individual preferences may exist for one or the other of these two types of judgement, and that corresponding to these there may be variations of efficiency.

(4) The writer is of opinion that these materials and tests constitute a reliable measure of tactual sense.

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A TWO-FACTOR STUDY OF SIMPLE MOTOR TESTS WITH PARTICULAR REFERENCE TO PRACTICE AND PROGNOSIS

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  (1) *Making a rough scale for converting 'raw' scores into a measure of the common factor* (pp. 419–420).
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I. INTRODUCTION

A PREVIOUS paper by the present writer (1) announced the discovery of a common factor in several simple tests of manual dexterity. This was of theoretical interest because earlier workers in the same field had frequently concluded that the ability to perform simple manual tests was specific to each operation. It was of practical importance because it was also shown that this common factor entered into many light repetitive operations in industry where its measurement was found to be of definite value in the prognosis of efficiency. What is in all probability the same factor in manual dexterity has since been noted by Attenborough & Farber (2), and also by Cox (3) in his recent outstanding work on manual skill. None of these studies, however, deals with the behaviour of the common factor in relation to practice in the abilities concerned. In view of the fact that motor tests usually exhibit pronounced practice effects, it was thought that the investigation of this problem might produce results of interest both in theory and in application, and the experiment described here was therefore planned.

II. THE PRACTICE EXPERIMENT

(1) *Tests, subjects and procedure*

The following tests were practised by a class of 39 girls and a class of 33 boys. Both groups attended a London Elementary School, and the ages were between 13 and 14 years.

(*a*) *Match insertion.* The subject sat before a small board pierced with holes in each of which a match could be inserted. He took matches, one by one, from a box in which they were neatly arranged with heads all one way, and tried to insert as many as possible in the time allowed. The subject was instructed to start at the top left-hand hole and to work along each row from left to right. The score was the number of matches inserted in 90 sec.

(*b*) *Placing rings on a rod.* The subject took small rings, one at a time, from a bowl placed within easy reach of the hand. He slipped them as quickly as possible over a vertical rod placed in front of him. The score was the number of rings placed in 2 min.

(*c*) *Screwing bolts into nuts.* The nuts and bolts were those provided in any Meccano outfit. The unassembled nuts and bolts were placed in separate heaps before the subject who was instructed to pick up each nut with the left hand, and with the right hand to screw a bolt into the nut as far as possible. The score was the number assembled in 2 min.

(*d*) *Knotting string.* The subject was given a number of small pieces of string, each about 3 in. in length and of comparable tension. He was told to tie a single knot in as many pieces as possible. The score was the number done in 2 min.

(*e*) *Threading beads.* The subject was provided with a small bowl of beads, and a threaded needle was held in the right hand. Each bead was picked up separately and passed over the needle right on to the thread before proceeding with the next. The score was the number of beads threaded in 2 min.

The experiment commenced in the third week of January, and ceased in the third week of July in the same year. During this period the subjects made two trials of each test every week with the exception of holidays, doing one test each day of the school week. Individual apparatus was provided and the test was completed by the whole group at the same time. In order to eliminate any disturbance which might have been introduced by diurnal peculiarities, or by the possible transfer of practice, the day on which any single test was executed was varied systematically. Every subject who carried out the entire programme should have completed 44 trials in each test by the end of the experiment. Actually, absences were frequent, so that, in order to preserve a group of useful size, it was found necessary to confine treatment of the records to the first 34 trials in the case of the girls, and the first 28 trials in the case of the boys. The records of subjects who failed to make these minimum attendances were neglected, this procedure reducing the group of girls to 33 individuals, while the boys' group was reduced to 29. These populations are of course not so large as are desirable for the analysis of test relationships. It was found impracticable, however, to obtain larger homogeneous groups of subjects who would all be available for testing at the same time. This, together with the wastage due to absenteeism in the course of such a protracted experiment, makes the use of comparatively small groups the only alternative to avoiding the problem altogether.

(2) *Practice curves*

Examination of the records of each successive trial, which would result in an unwieldy mass of data, seems quite unnecessary. What has been done is to calculate the mean score for each group in each test at stages of practice corresponding to the 1st, 7th, 12th, 18th, 23rd, 29th and 34th trials. Inspection of the detailed records showed that the second was usually the better of the two trials given at each practice period, but not invariably so. Sometimes a score was made unreliable by a subject's having dropped some of the material or by some other mischance. Consequently, it seemed better to represent any practice stage '*n*' by the best score made in trials '$n-1$', '*n*', and '$n+1$'. That is, the initial score of any individual was taken as the best score made during the first three trials, and after that his score corresponding to stage 7 was his best score made in trials 6, 7 and 8, and similarly with the later stages. Practice curves based on these measures are shown in Fig. 1.

The curves reveal a very interesting state of affairs. It will be noticed that the initial scores were approximately the same for both groups in every test except one, but, while the girls show a progressive increase in ability, all improvement by the boys has ceased by the 18th, and in most tests by the 12th trial. It seems that this difference can only be explained by reference to the conative factors involved. No incentive was offered in the way of reward, and the teachers who supervised the experiment were asked not to exercise any pressure beyond reminding the children that it was important that they should do their best. The writer personally supervised the first application of the tests and gave each group a preliminary talk in which the aim of the experiment was roughly explained and the importance of maximum endeavour was emphasized. This seems to have been sufficient to secure the desired results in the case of the girls, but the boys as a group appear to have lost interest at a comparatively early stage, and their records have therefore been neglected in the further treatment of these figures. This conclusion seems unavoidable in view of the consistent evidence of all five curves, thus illustrating the importance of incentives in this kind of work.

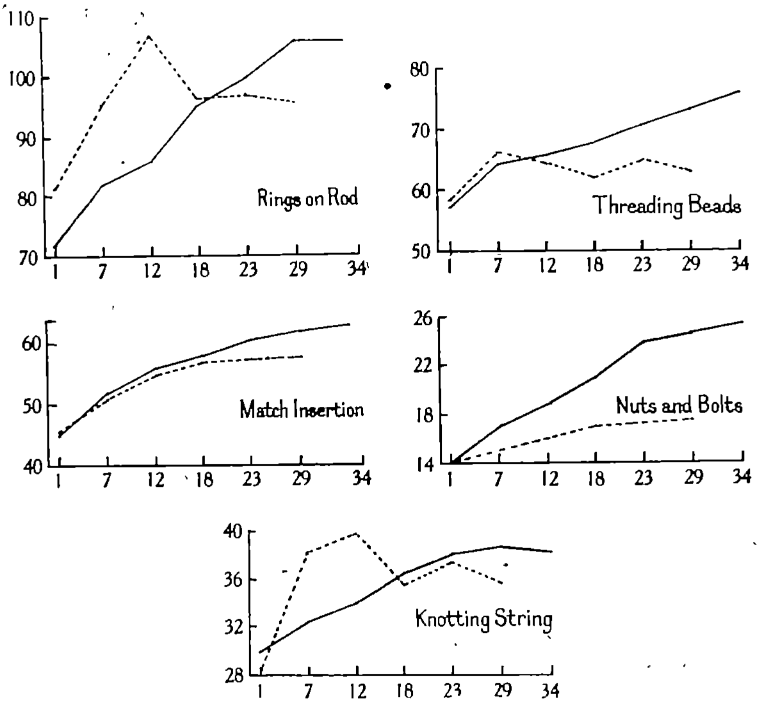

Fig. 1. Curves showing effects of practice in tests of dexterity.
Girls—— Boys......

(3) *Analysis of correlation between tests at various stages of practice*

The intercorrelations of the five tests practised by the girls have been calculated for stages of practice corresponding (in the way already described) to the 1st, 12th, 23rd and 34th trials, and are shown above the diagonal in sections, 1, 2, 3 and 4 respectively of Table I, where *a*, *b*, *c*, *d* and *e* denote the tests described on p. 412. Coefficients greater than 0·35 would occur by chance less than 5 times in 100.

The first step in the analysis of these correlations was to calculate the saturation of the tests in each section in the assumed common factors g_1, g_2, g_3 and g_4. This was done by means of the formula given by Spearman (4):

$$r_{ug} = \frac{(A^2 - A')^{\frac{1}{2}}}{(T - 2A)^{\frac{1}{2}}}.$$

The results are shown in heavy figures below each section of Table I.

The next step was to use these values to calculate the partial correlation between each pair of tests when the factor assumed to be common in each section is held constant. This was done by means of the usual formula:

$$r_{ab.g} = \frac{r_{ab} - (r_{ag} \cdot r_{bg})}{(1 - r^2_{ag})^{\frac{1}{2}} \cdot (1 - r^2_{bg})^{\frac{1}{2}}}.$$

The residual correlations after the partialling-out of the common factor assumed at each stage of practice are shown in heavy figures below the diagonal in each case. It will be seen that none of these is significant, the greatest residual being a value of 0·32 occurring once in section 1 and once in section 4, and, as such a magnitude may be expected by chance in 10 per cent of cases, it appears that the intercorrelation in each section may be ascribed without great error to a factor common to all the tests in that section.

The amount of the total variance which can be ascribed to the common factor at each stage of practice is proportional to the mean of the squared saturations of the individual tests in the common factor, and this value $\left(\frac{\Sigma r^2_{ug}}{5}\right)$ is shown in heavy figures below each section. It will be seen that it remains constant, or, if anything, shows a slight tendency to increase. From this it may be inferred that the importance of specific factors does not increase with practice.

It now remains to discover if the common factors at each stage of practice (g_1, g_2, g_3 and g_4) can be identified with each other. This has been

Table I

Section 1

	a_1	b_1	c_1	d_1	e_1
a_1	—	34	31	50	46
b_1	01	—	41	36	52
c_1	−20	−02	—	51	73
d_1	20	−05	04	—	45
e_1	−04	07	32	−27	—
r_{ug_1}	57	58	73	67	84

$\frac{\Sigma r^2_{ug_1}}{5}=0{\cdot}47$

Section 2

	a_2	b_2	c_2	d_2	e_2
a_2	—	16	48	34	35
b_2	−19 $(-07)_1$	—	54	56	65
c_2	24 $(-16)_1$	−05 $(52)_1$	—	53	66
d_2	06 $(12)_1$	17 $(51)_1$	−15 $(41)_1$	—	50
e_2	00 $(-23)_1$	26 $(64)_1$	−06 $(32)_1$	−17 $(37)_1$	—
r_{ug_1}	71	29	78	37	66
r_{ug_2}	43	67	84	70	81

$\frac{\Sigma r^2_{ug_1}}{5}=0{\cdot}35$

$\frac{\Sigma r^2_{ug_2}}{5}=0{\cdot}50$

Section 3

	a_3	b_3	c_3	d_3	e_3
a_3	—	41	48	35	48
b_3	−06 $(13)_1$	—	59	42	63
c_3	02 $(17)_1$	00 $(41)_1$	—	52	54
d_3	00 $(15)_1$	04 $(27)_1$	10 $(40)_1$	—	42
e_3	06 $(12)_1$	14 $(43)_1$	−15 $(26)_1$	−06 $(22)_1$	—
r_{ug_1}	61	53	56	40	68
r_{ug_3}	59	75	79	59	76

$\frac{\Sigma r^2_{ug_1}}{5}=0{\cdot}32$

$\frac{\Sigma r^2_{ug_3}}{5}=0{\cdot}49$

Section 4

	a_4	b_4	c_4	d_4	e_4
a_4	—	62	46	33	53
b_4	$(33)_2$ 26 $(45)_1$ $(17)_3$	—	63	27	70
c_4	$(16)_2$ −11 $(17)_1$ $(-27)_3$	$(42)_2$ 05 $(51)_1$ $(31)_3$	—	48	71
d_4	$(14)_2$ −02 $(27)_1$ $(-08)_3$	$(04)_2$ −24 $(20)_1$ $(-03)_3$	$(36)_2$ 12 $(44)_1$ $(28)_3$	—	59
e_4	$(04)_2$ −25 $(08)_1$ $(-21)_3$	$(40)_2$ −04 $(58)_1$ $(43)_3$	$(52)_2$ −14 $(58)_1$ $(42)_3$	$(53)_2$ 32 $(62)_1$ $(44)_3$	—
r_{ug_1}	73	48	49	21	67
r_{ug_2}	65	66	56	36	78
r_{ug_3}	83	66	69	44	73
r_{ug_4}	64	76	80	53	93

$\frac{\Sigma r^2_{ug_1}}{5}=0{\cdot}30$

$\frac{\Sigma r^2_{ug_2}}{5}=0{\cdot}38$

$\frac{\Sigma r^2_{ug_3}}{5}=0{\cdot}47$

$\Sigma r^2_{ug_4}$

done by taking tests a_1, b_1, c_1, d_1 and e_1 as reference values and calculating the correlation (r_{ug_1}) between the common factor g_1 and each test at successive stages of practice. Owing to the possibility of specific correlation between repetitions of identical tests, the usual formula was modified by the omission of such instances and was used in the following form which shows the correlation between test a_4 and the common factor of section 1:

$$r^2_{a_4 g_1} = \frac{r_{a_4 b_1}(r_{a_4 c_1} + r_{a_4 d_1} + r_{a_4 e_1}) + r_{a_4 c_1}(r_{a_4 d_1} + r_{a_4 e_1}) + (r_{a_4 d_1} \cdot r_{a_4 e_1})}{r_{b_1 c_1} + r_{b_1 d_1} + r_{b_1 e_1} + r_{c_1 d_1} + r_{c_1 e_1} + r_{d_1 e_1}}.$$

These coefficients, giving the saturation of each test in the factor g_1, are shown below each section in Table I.

The next step was to use these values in the formula for partial correlation to calculate the residual correlation between each pair of tests when the factor g_1 is held constant. These residuals are shown below the diagonal in each section and are printed in parentheses—$(\ )_1$. It will be seen that considerable residuals are much more frequent than would be expected by chance, thus showing that the factor g_1 is not a sufficient explanation of the intercorrelation in sections 2, 3 and 4. Values of $\frac{\Sigma r^2_{ug_1}}{5}$, representing the proportion of the total variance which can be ascribed to the factor g_1, are also shown in each section. The significance of differences between these proportions remains unknown, but these latter seem to follow a diminishing tendency throughout the period of practice.

Finally, the same methods have been used to show the residual correlation remaining between each pair of tests in section 4 when each of factors g_2 and g_3 is held constant. These coefficients are shown in parentheses $(\ )_2$ and $(\ )_3$ respectively. Here again it is clearly shown that neither of these factors suffices to explain the intercorrelation in section 4. The proportion of the total variance in section 4 which can be ascribed to each common factor of preceding stages is also shown, and appears to follow a definite tendency to increase.

(4) *Conclusions*

The preceding analysis has shown that the intercorrelation at each stage of practice can be looked upon as due to one common factor appropriate to that stage, but that these factors are by no means identical. They do, however, have much in common in view of the fact that g_1 contributes considerably to the variance of each test at the three subsequent practice stages, and that factors g_2 and g_3 each contribute largely to the variance at the later stage of practice.

These results point to the conclusion that each of factors g_2, g_3 and g_4 consists in reality of at least two factors, one of which is g_1. This does not of course affect the practical usefulness of resolving the intercorrelation at each of these stages into a single common factor together with specifics; a step of which the validity is shown particularly well by the negligible residuals printed in heavy figures in section 3. It does, however, show very clearly that a common factor between tests at one stage of practice cannot be identified, without experimental evidence, with the common factor which may be demonstrated to unite the same tests at an earlier or later practice stage. It has been noted, however, that the proportion of the total variance at stage 4 which can be ascribed respectively to factors g_1, g_2 or g_3 appears to increase progressively. From this it can be inferred that the part of the total communality at any stage which is not due to g_1 becomes more nearly the same as practice progresses.

Up to this point no psychological significance has been assigned to any of the factors indicated by the preceding analysis. This will now be attempted. It has been shown that the communality at each stage of practice may be ascribed to a factor which is composed of g_1 together with another factor (or factors) which is not the same at different stages, although it tends to become more stable as practice continues. Of course, the factor g_1 may itself be complex in actual fact, but this does not affect its appearance and use as a unitary factor within the limits of this analysis.

Bearing all this in mind, it seems to the writer that it is reasonable to identify the factor g_1 with something in the nature of manual dexterity, and to ascribe to conation all that part of factors g_2, g_3 and g_4 which is not covered by g_1. This assumption neglects the possibility that g_1 may actually consist of a more fundamental factor together with a conative factor, but there is good reason to believe that zeal would have little weight as a determiner of individual differences during the first few trials when interest has been aroused. It would be expected, however, that this factor would become increasingly important during the course of a prolonged experiment, and, moreover, that its effective nature would vary until sufficient time had elapsed for the 'triers' to have become definitely separated from those whose interest had not been maintained. Such a situation would agree very well with the data presented in Table I and with the importance of conation as illustrated by the curves shown in Fig. 1.

Finally, there remains a matter of some importance in the practical application of tests such as these. If the preceding analysis is correct, it might well be asked whether any useful purpose could be served by the application of unpractised tests in which the common factor becomes increasingly less important as a determiner of individual differences in the same tests at later stages of practice. Similarly, it might be argued that the tests would not be reliable instruments unless they were applied after the attainment of that stage of practice sufficient to produce relative stability of the factors concerned, and this, it has been shown, would involve so many trials as to be impracticable. Each of these criticisms loses much of its weight if it is remembered that the tests measure at least two factors, one of which has been assumed to be manual dexterity while the other has been assumed to be conative. Reason has been given for believing that it is the first factor which is measured most adequately by the unpractised tests, and, as this is the more important factor in industrial prognosis, what might have been a serious difficulty tends to disappear. This conclusion is supported by the following experimental evidence.

III. APPLICATION OF THE TESTS IN A FACTORY

(1) *Making a rough scale for converting 'raw' scores into a measure of the common factor*

For practical application it is necessary that the 'raw' scores actually made by any subject in a series of tests should be readily convertible, without the labour of statistical calculation, into a composite score expressing his relative endowment in the function under discussion. An attempt was made to meet this requirement by treating the test scores of 70 girls attending a Day Continuation School attached to a large factory. It is true that a much larger number of subjects is necessary for the standardization of any test, but this group was very satisfactory from many points of view, being composed entirely of girls between the ages of 14 and 15 years, all having little or no industrial experience, while the conditions of the testing were likely to produce reliable results.

The tests used were those practised by the school children and described on p. 412. The first step in treating the 'raw' data was to calculate the correlations between the tests. These coefficients (which are omitted from this paper in order to reduce the number of tables) had an average value of 0·41, and were satisfactorily significant, the lowest being nearly three times its probable error, and nearly all the rest being greatly in excess of this ratio. The correlation between each test and the assumed common factor (r_{ug}) was calculated, and the resulting values were used in the formula for partial correlation to determine the degree of residual correlation when the common factor is held constant. This was satisfactorily small in every case but one, and it was therefore assumed that little error would be introduced into subsequent calculations by ascribing all the original correlations to a single common factor.

When this assumption is made, Spearman's (4) formulae can be used to determine what weight should be given to each test to produce the highest possible correlation between the weighted pool of tests and the factor common to them all. The weight is given by the formula:

$$w_u = \frac{r_{ug}}{1 - r^2_{ug}},$$

where r_{ug} stands for the correlation between the test and the common factor. The correlation, r_{pg}, between the best weighted pool and the common factor is then obtained from the formula:

$$r_{pg} = \frac{1}{\left(1 + \dfrac{1}{S(w_u \cdot r_{ug})}\right)^{\frac{1}{2}}}.$$

These calculations show that the team of tests, when weighted appropriately, has a correlation of 0·90 with the common factor, and this suggests that it would be useful in discovering the relative endowment of different individuals in this function. Before the team of tests can be used conveniently for this purpose, however, it is necessary to have some means of transforming the actual test scores into standard weighted scores, which are directly comparable, without the labour of statistical calculation each time the tests are used. In order to obtain such a scale of reference, it was assumed that the group of 70 subjects represented a fair sample of girls aged between 14 and 15 years in the performance of these tests. The standard weighted score was then calculated for each individual, and by placing this series of measures alongside the actual 'raw' scores, a rough scale was drawn up which showed the value of the standard weighted score corresponding to the 'raw' score made by any subject in each test. The scale is omitted from this paper as it is derived from too small a sample to be sufficiently reliable for serious vocational testing. That it has some value, however, was shown by its subsequent use to predict industrial proficiency in several light repetitive operations.

(2) *Use and value of the scale in forecasting industrial proficiency.*

The first test was made on the records of 21 girls, aged between 14 and 16 years, engaged in wrapping toffee by hand. These girls carried out the tests of match insertion, placing rings on a rod, and knotting string. Their scores in each of these tests were transformed into weighted scores by reference to the scale. Each girl was then ranked according to her total weighted score, and these ranks were compared with the ranking for industrial proficiency as measured by the time taken to wrap 40 slabs of toffee. The correlation between the two rankings is 0·49. It is therefore evident that the scale has some predictive value in this instance.

This conclusion was confirmed by applying the same procedure to the test scores of 77 girls engaged in light repetitive work in a chocolate factory. The girls were taken from four departments of which three were employed in an operation known as 'nutting'. This is a simple operation in which the essential feature is the accurate placing of a prepared nut on a chocolate base. The other department was engaged in wrapping a chocolate assortment in tinfoil covers, all the work being done by hand.

The girls in each group were ranked in respect of their 'dexterity' scores, obtained by converting the actual test scores by reference to the scale, and also according to their ability as measured (*a*) by a sample test of their work as observed for a period of roughly 10 min. under favourable conditions without interruptions, and (*b*) by the personal estimate of the forewoman in charge. The resulting intercorrelations are shown in Table II.

Table II

Operation	No. of subjects	Tests *v.* sample	Tests *v.* forewoman's estimate	Sample *v.* forewoman's estimate
Foiling	16	0·32	0·43	0·49
Nutting A	20	0·31	0·38	0·29
Nutting B	21	0·20	0·51	0·85
Nutting C	20	0·37	Team work	Team work

It will be seen that the tests are positively related to the sample test of industrial performance in all groups, and that this relationship is appreciable although not high, increasing in size when the tests are compared with the measure of ability provided by the forewoman's estimate. In general, if the forewoman's estimate is taken as the criterion, the workers are differentiated in respect of ability nearly as well by the tests as they are by a short sample test of their actual work.

(3) *Conclusions*

It must now be emphasized that the tests were performed once only by the girls whose scores were used to draw up the rough scale. The scores of the confectionery workers were also derived from a single application of the tests, and in both cases there was no previous practice beyond the minimum necessary to ensure that the experimenter's instructions had been understood. The results which have just been presented therefore show that the unpractised tests have some industrial significance despite the fact that their common factor at this early stage was found to have increasingly less importance as a determiner of individual differences in scores on the same tests at later stages of practice. This agrees very well with the view, suggested earlier in this paper, that the common factor in the unpractised tests is almost entirely determined by manual dexterity, whereas the maintenance of interest and the will to make a good score become increasingly important causes of individual differences with the lapse of time.

Finally, it is worth repeating that the rough scale, used to convert the 'raw' scores into the measures of dexterity which were correlated with workshop efficiency, was derived from the test records of a totally different group of girls, of the same age but engaged in a different occupation. The fact that the use of this scale has in five cases produced a series of measures which correlate positively with occupational ability may be regarded as the best sort of proof that the tests have adequate reliability. It may also be interpreted as evidence that the common factor corresponds to some psychological function of general importance, and that it is not a mere statistical property of the test scores of any particular group of subjects.

IV. SUMMARY

1. Five tests of manual dexterity were practised by a group of boys and a group of girls. The girls showed consistent improvement, while the boys ceased to improve about half-way through the course of the experiment. The difference was ascribed to failure on the part of the boys to maintain interest in their progress.

2. The intercorrelations of the girls' scores were examined at four stages of practice, at each of which a single common factor was found sufficient to explain the coefficients.

3. At each stage the common factor was found to be responsible for roughly the same proportion of the total variance, thus indicating that

the importance of specific factors had been relatively unaffected by practice.

4. The common factors at each separate practice stage could not be completely identified with each other, and the common factor in the unpractised tests was shown to be less important as a determiner of individual differences at later stages of practice.

5. The tests were applied once only to a group of girls whose scores were then used to draw up a rough scale for converting 'raw' test scores into standard weighted scores. This scale was used to convert the 'raw' scores derived from a single application of the tests to five groups of factory workers. The measures of manual dexterity so obtained were positively related to workshop efficiency in each case. The value of the unpractised tests was therefore established.

6. It is suggested that the experimental results are most satisfactorily explained by the assumption that the common factor in the unpractised tests is almost entirely determined by something in the nature of manual dexterity, and that the same factor is associated with a conative factor to determine the communality at later stages of practice.

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PSYCHOLOGICAL ASPECTS OF CONTEMPORARY POETRY[1]

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I. *Methods of approach* (pp. 425–426).
II. *The obscurity of modern verse* (pp. 426–428).
III. *Speech as the medium of verse* (pp. 428–430).
IV. *Affective meaning and speech* (pp. 430–432).
V. *Modern poetry and 'communication'* (pp. 432–435).

I. METHODS OF APPROACH

WHILE preserving, as he must, a strictly existentialist position, and avoiding the judgement of value, there are three points of view from which the psychologist may approach the problems presented by art in general, or by poetry in particular. Taking first of all the content of the poems, the substantive product of the artist, he may consider in their qualitative aspect *the nature of the symbolizations employed.* In accordance with this first approach the psychologist may then attempt to draw conclusions concerning the characteristics of the individual artist or the nature of the artistic process. This *forme fruste* of the clinical method has been commonly employed by those writers who have built their theories upon a groundwork of psycho-analytic thought. The potentialities of the method, however, are ordinarily more limited than its practitioners allow, since the kind of interpretation which it attempts to give of the artist or of the process of creation, requires, as soon as it leaves the field of broad generalization in terms of some theory of types or of common symbols, to be corroborated or validated by rather full knowledge of the life history of the individual artist. But such data are seldom, if ever, available in sufficient detail. It will be well, however, to bear in mind the lines of generalization indicated by this attack upon the problem, and to remember that what we commonly call the psychology of art is concerned always with the psychology of the artist or of his audience, and not with the artistic product itself, except in so far as that is the source of the available data.

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[1] The substance of this paper was presented at the meetings of the British Association, Section J, at Norwich, 1935.

Secondly, the psychologist may concern himself with the problems of the artist in so far as he is a member of the particular *milieu* of contemporary society. Perhaps it is this *social psychology of art* that has been most neglected in the past, the psychologist, far more frequently than the critic, having been all too ready to consider the artist, if not as a *rara avis*, at least in isolation from his environment.

The third point of departure is to be found in a consideration of *the psychological nature of the medium* itself, of the psychological problems that inevitably arise in its employment, and of the psychological characteristics of a particular artist, or group of artists, that become apparent in their manner of exploiting it, in such a way and to such an extent as to differentiate them for the reader. It is this point of view that is implied, albeit in terms of a somewhat inadequately formulated psychology, in the critical writings of Mr I. A. Richards and his followers. The value of it is at once apparent when we notice that it is the line of approach that leads most directly to a consideration *in the same terms* of the psychological problems not only of the artist, but of his audience as well, and that it enables us, by way either of generalization or of contrast, to bring the psychological characteristics of poetry within our broader formulations in respect of the psychology of speech or language. It is from this point of view principally, in order to elucidate certain points of disagreement with some recent considerations of contemporary poetry along similar lines, that the present discussion starts.

II. THE OBSCURITY OF MODERN VERSE

From its origins in Aristotle, literary and artistic criticism has commonly, and not always unwittingly, attempted psychological interpretation as well as aesthetic evaluation. In critical discussion of modern poetry there has been an increasing tendency to emphasize the former line of approach at the expense and sometimes almost to the exclusion of the latter. This has been the result only in relatively slight degree of the direct infiltration of influences from contemporary psychology into the modes and matters of thought of the poets themselves, although these influences have been significantly at work, and sometimes not least emphatically when there has been explicit disavowal on the part of the writer. It is by reason of its characteristics of expression and style, rather than of experiential content, that modern poetry appears directly to demand psychological interpretation.

Of these characteristics the bewildering obscurity of the verse is the one which has most frequently appeared to call for explanation, if not for extenuation, in psychological terms. It is that obscurity which provides the initial challenge to some form of deeper analysis which may reveal the factors that must be supposed to underlie the idiosyncrasy of the verse. If we take as our starting-point the poems themselves, there can be readily distinguished certain stylistic features which make their appearance in greater or less degree in the work of all the writers of the group of which Mr T. S. Eliot and Mr Ezra Pound must in some sense be regarded as the progenitors. Summarizing these for the purpose of our discussion briefly and somewhat too dogmatically, we find that the writing is in general paratactic rather than syntactic, the common forms of grammar being absent, obscured, or reduced to a narrower range of simple types than is usual when speech has undergone the degree of formalization imposed by writing. Even when this is not superficially apparent in the grammatical structure of the verse, parataxis is the customary mode of the sequence of thought. Manifest coherence is at a discount, many seemingly essential words being omitted. Conjunctions, especially those indicating the subtler or more complex logical relationships, or providing clues to the 'direction' of the process of thought, are few in number. Instead of subordination, we commonly find relatively simple juxtaposition or succession, so that the completed product is in appearance peculiarly associational and lacking in superficial 'wholeness' or unity.

Excepting in the term associational, there is here little attempt at psychological interpretation, but merely an analysis, primarily from the linguistic point of view, of the generalized obscurity of the writing. But it must be recognized that this original judgement of obscurity could be made only in virtue of certain anticipations on the part of the reader—the anticipation, namely, that the printed poem will, if he bring to it a degree of intelligence, sensitivity, and effort of attention, communicate something to him. Round this problem of communication, of the justification for those anticipations with which the reader approaches the poem, rages much of the contemporary discussion of aesthetics. On some theories communication is given a position of central importance; on some, where the artistic motivation is primarily in question, its significance is minimized; sometimes it is denied or treated as incidental. Now we need not here confuse the issue by attempting to find a general formula for the arts other than poetry. With this proviso, the problem is so considerably narrowed that our position immediately suggests itself. The poet employs and exploits the medium of speech, and in so doing he inevitably arouses in his reader the anticipation of communication. Whatever theory of expressionism he or his critics may adopt in the effort after solipsistic rationalization of a poetic practice which fails to communicate, the very act of writing implies some sort of audience, however distant or ideally conceived.

III. SPEECH AS THE MEDIUM OF VERSE

It is of set purpose that we have spoken of the poet as employing the medium of *speech*, although we more commonly think of poetry and discuss it as art in the medium of language. This popular usage is in part responsible for the sleight of hand by which in psychological discussion we tend to equate the terms speech and language, allowing thereby to become obscured the peculiar characteristics of both. Speech, as Gardiner points out, is a *nomen actionis*, which may be as such behaviouristically considered; whereas language is a collective name for a system of linguistic facts, or, if we may adapt a phrase from G. F. Stout, it is a somewhat unstable system of conditions which circumscribes within more or less shifting limits the employment of the elements of speech. Now writing must be regarded as a genuine, though secondary, form of speech, and a sound psychological theory must take account of the attributes and functions which they have in common. Of these functions the fundamental one is that of communication, immediate or delayed, which results —whether or not as the outcome of conscious intention—in the production of some effect in an audience, near or remote. This effect is ordinarily such that it can be satisfactorily rationalized by taking account of the speech which was its occasion. In so far, then, as this modernist poetry is in greater or less degree obscure—in so far, that is to say, as it fails to communicate—it appears to be employing its medium in a way which, independently of our opinion as to whether the attempt is foreign to the medium, is differentiated *psychologically* from the customary modes of exploitation of it.

In extreme cases like the work of Miss Stein, which are separated from the rest, perhaps not only in degree but also in kind, the writer, even when preserving a grammatical schema, seems to have discarded the medium of speech in the endeavour to resort to language merely, to have made a tactical retreat into a region apart from competition with the arts of communication. Hence her device of bombarding us with names, repeated in such a way as to restrict them to the bare denotation of the words, or arranged in such collocations that even this last shred of contact between writer and reader has disappeared. Hence, too, her uncertain verbalization of the distinction between prose and poetry: "that is poetry really loving the name of anything and that is not prose". In work of this kind we seem to have passed the bounds even of monologue, and it may be that questions of lucidity and obscurity, of communication or the reverse, do not arise at all.

More commonly, however, these are the very questions which are most insistent.

> I grow old...I grow old...
> I shall wear the bottoms of my trousers rolled.
> Shall I part my hair behind? Do I dare to eat a peach?
> I shall wear white flannel trousers, and walk upon the beach.

In lines like these the reader can see what the poet is saying; it is less easy for him to *see through* it to what the poet means. This surely is the heart of our problem, the crux of meaning. Superficially the meaning of the lines from Mr T. S. Eliot seems to be clear, since the sense, the things to which they refer, is obvious. We can follow without difficulty the denotations not only of the words but of the sentences employed. But we remain uneasily aware, since the matters dealt with are in themselves so trivial, that there is something that we have failed to apprehend, something other than the bare denotations of the words, or the reference of the sentences. We must assume that these questions and assertions had some meaning for the writer, that they were, that is to say, the expression of some more or less specific mood, in which some situation had itself found meaning, and in virtue of which they took their present form. This meaning-for-the-writer is not to be equated with 'intention', since intention to communicate, or intention to produce an effect of some kind upon an audience, is merely one factor conditioning the verbal formulation of meaning. The affective content which we have been calling meaning-for-the-writer, and not the intention, nor the asserted fact that Prufrock proposes to wear his trousers in the fashion specified, is the essential meaning of the utterance, the matrix from which the line takes its rise. This is the same general position that has been repeatedly enunciated by Mr Richards. It seems, however, that he has been misleading in his discussion of the relation between this affective meaning and the cognitive denotation of the statements involved, and in particular in his suggestion of an almost absolute dichotomy between the two uses of language, which he calls on the one hand exactly enough, emotive; on the other somewhat loosely, since the distinction which he draws is basically a psychological one, scientific.

IV. AFFECTIVE MEANING AND SPEECH

The nature of speech in its affective aspect is ordinarily obscured by our failure to distinguish between speech and language, and by our discussion of language in terms of the concept pure. The typical linguistic element labels a concept, but, as Sapir points out, "it does not follow that the use to which language is put is always, or even mainly, conceptual, and even where each element in a sentence defines a separate concept, or conceptual relation, the sentence as a whole may have no kind of conceptual significance whatever". Apart from the fact that the concept in the concrete, as the individual thinks it, is always accompanied by some affective factor, it is more important for our purpose to notice that in expression and communication the speaker's intention or emphasis, and the reader's attention, may be directed in all degrees from all-but-zero to all-but-entirety on one or other aspect of the utterance. But it is never all-or-none, never all emotional, or all cognitive. In a scientific statement the writer is concerned primarily with exact and specific denotations. But the customary human modes of expression and communication have no means of dealing with things (ideas or objects) completely stripped of their importance for human interests. There is not another totally different use of language in which the writer is concerned, irrespective of the denotations of the words, to communicate a state of feeling. It is possible to do even this, except perhaps at the very lowest linguistic level of Hughlings Jackson's 'detonating commas', only by manipulation of the verbal denotations to the desired end. Connotation is logically, and psychologically still more emphatically, secondary to and dependent upon denotation. In our discussions of metaphor we are apt to lose sight of this fact.

It has been pointed out that in speech it is we, and not the words, that mean, and nearly always we mean more than the mere words convey. But the things we say have meaning—'sense' or reference—in themselves, and it will depend upon the hearer's or reader's part in, and understanding of, the total situation, how far he takes them at their face or denotative value, and how far he chooses to look behind or through them for an emotional content. It is commonly accepted that in the young child the conative and affective aspects of speech are predominant, and those who are in daily contact with him respond as if the element of desire or rejection were always present, even when it is not specifically verbalized. Now, although the development of speech implies the interacting processes of socialization and intellectualization, many of the utterances of the adult are not different from the genetically more primitive type. The "I-shall-wear-the-bottoms-of-my-trousers-rolled" of the disillusioned Prufrock, like most of the assertions of poetry, is a case in point, in which the implicit affective background far outweighs the trivial references involved. But the feeling is communicated, so far as it is communicated at all, by a denotative utterance, by an assertion in which the affective 'thing-meant' is not directly mentioned.

The range of feelings or attitudes that can be communicated or aroused without this element of denotation is extremely narrow. The groan of sorrow, the shriek of pain, the shout of joy, the cry of anger we have, but the occasions on which emotion rises to such a pitch of ejaculatory purity or intensity are few. Direct statement of the more complex emotional states is liable also to fall short, since the linguistic means available to the individual are never adequate to the potential range of his experiences. Moreover, in poetry, whenever the acoustic effects are not of a somewhat obvious—which is not to say, crude—kind, the cognitive element of denotation is more, and not less important than it is in ordinary speech, since the auxiliary elements of intonation and gesture, and the context of a concrete situation, are wanting. Yet it is by a reduction, commonly in the definiteness, frequently in the elaboration, and sometimes in the quantity of denotative material, that modernist poetry proceeds. This is effective in widely varying degrees as we pass from the work of Mr T. S. Eliot at the one end to that of, say, Mr E. E. Cummings at the other.

The practice appears to be inspired by, when it does not explicitly implement, a false theory of verbal suggestion and an underlying misconception of the psychological characteristics of the linguistic machinery. In its extreme form, as enunciated by Valéry, the principal spokesman of the French *symbolistes*, the theory asserts as the aim of the poet the elimination of all the intellectual elements, and the retention of 'rien que la suggestion', to make way for the direct induction of a 'mental state' in the reader. It fails to consider that suggestion takes effect only when someone makes a suggestion, and that this is almost wholly impossible without some cognitive specification. Even at a very low level, the cry which elicits a response is 'understood', though the cognition need not be explicit, as having as its reference some individual pain, or anger, or joy. There is no way of arousing in another, by means of language, any kind of mental state whatever, when all cognitive reference has been totally eliminated. This is not to say that, in the communication of emotion, the cognitive reference must be analytically apprehended as such, nor is it to deny the fact that a too literal attention to reference may impede the development of the emotional response. But we cannot dispense wholly with the reference. In the psychiatric employment of the association word list, responses that go deeper than mere verbal association are touched off, not by the word, but by the 'thing-meant' by it, or in virtue of some selection from its cognitive references. It is by reason of the fact that certain persistent attitudes, or complexes, of the subject have acted as a logical *prius* to delimit and canalize these references that the stimulus word can be used as a psychological probe.

V. MODERN POETRY AND 'COMMUNICATION'

Returning now to the characteristics of modern poetry which we have already noticed—its paratactic linguistic structure and its apparent avoidance of relational verbal synthesis—we shall find that it is in the matter of reference that they principally bear upon the problem of communication. There is in the development of speech in the child a stage of relatively pure parataxis, which is adequately represented by the examples given by Jespersen: *Out pull baby specs*, or *Oh papa lamp mother boom*. Without the specification of the situational context, the latter of these at least is fairly incomprehensible. Add now to paratactic structure the further complication of a wide vocabulary in which every word is rich in potential references and associations, and the confusion is complete. Psychologically considered, the forms of syntax have been developed so that each sub-unit of speech is employed to delimit, rather than to enlarge the field of reference of every other within the same complex whole. Any piece of connected verbal communication consists of several references which may be thought of as acting upon each other at different levels. In part determining, and in part over-determined by the reference of any completed unit of speech, such as the sentence, is the denotation of each of the particular words within it. A similar condition of interaction holds between the reference of each such part-whole and that of the poem, paragraph, or other larger unit in its entirety. If the words are brought together, as is not infrequently the case in modern poetry, in apparently unrelated juxtaposition, they will have no reference because they have too many references. Each word, as Gardiner describes it, has a certain area of meaning, or group of references lying at different distances from one central or generally accepted denotation. Communication is possible only in so far as we can specify fairly exactly just which of these denotations, with its accompanying associations, immediately concerns us. The degree of specification may vary. In scientific discourse it is ordinarily limited to only one of the references of the words employed, that reference being given its full conceptual value. In poetry the specification is less complete. Within the apperceptive context built up through the phrase, the sentence, or the larger unit as a whole, one reference will be dominant. But certain others may be retained for their associative value—because, since they already belong to other apperceptive systems, they have the effect of bringing within the poem something of the affective colouring that belongs to these systems. But this retention of 'overtones of reference' is not, as some have implied, the same thing as providing us with an unlimited choice of references or associations, whether generally accepted or wholly individual.

Several possibilities present themselves of explaining or describing in general psychological terms these characteristics of modern poetry. It might be that the apparent parataxis is not properly paratactic at all, but is simply associational in the strict sense, as Mr Sparrow believes it to be. Such an association train would be without 'sense', and therefore without meaning discernible by the ordinary reader. But those poems in which the technique is most obviously associative, like Mr Eliot's *Rhapsody on a Windy Night*, are not always the least comprehensible, nor is the chain of association even in them completely free. In the *Rhapsody* we start with a certain situation explicitly indicated in the title of the poem. This may have been the occasion of the mood in which the poem originated, but it may equally well have been the creature of that mood, the means by which it could best by symbolized and projected. By the mood and the probably fictitious topic together, the associations are constrained and directed, and the temporal framework of the poem is utilized to provide, independent of linguistic links, a means of transition.

In speech or writing, the process of association is ordinarily in some degree constrained—by some vague Aufgabe or preconception of purpose, or by a relatively clear, though not necessarily verbalized prevision of the whole, such that expression appears to have the character of the Wundtian analysis. In the moment of expression, that is to say, intention and attention are directed upon a particular object, the 'thing-meant', or the topic in hand. The case is not essentially different when the thing-meant, the matter to be communicated, is an attitude or mood, except in so far as this may itself condition more directly and emphatically the associations that occur. If the mood has a high degree of self-consistency and considerable intensity, and is not widely removed from common experience, it may well give rise to associations, the affective rationale of which, even in the absence of habitual linguistic or logical links, will be self-evident.

It appears, therefore, that we must go beyond association in order to account for the characteristics of modern poetry. In doing so we shall concentrate upon the two factors which, in ordinary communication, act as curb and rudder to the associative process, namely, the over-determining mood or attitude, and the conscious direction. When the latter is considerably in abeyance, then, whenever the changes of mood are abrupt, or the trend of its development unusual, we should expect to find abrupt, obscure transitions comparable to those which occur in the development of a dream, and which are incapable of rationalization even in the secondary elaboration, as described by Freud. In such a case the narrator himself is baffled to fill in the gaps in the façade, and, being unable to decide whether the bridge that is required should be of the nature of a 'because', a 'therefore', or a 'whereas', he is compelled to leave the unrelated elements loosely side by side. Now it must be remembered that, whatever sense of dissatisfaction this may occasion in conscious narration, there was none in the dream itself. W. H. R. Rivers has pointed out the similarity between these absurd, but during sleep apparently natural, transitions of the dream and those which are very common in the narrations of primitive peoples. "At one moment", to take Rivers's example, "the narrator will be talking about a man, and at the next he will speak of his sitting on the bough of a tree, as if it were a perfectly natural thing to do, it being quite unnecessary to make any reference to the transformation into a bird which, from our point of view, is necessary to make the story intelligible." Compare with this description an example drawn from Mr Eliot:

> Shall I say I have gone at dusk through narrow streets?
> And watched the smoke that rises from the pipes
> Of lonely men in shirt-sleeves, leaning out of windows....
> I should have been a pair of ragged claws
> Scuttling across the floors of silent seas.

In this analogy we seem to find a clue to the broadest generalization possible, on our approach from the linguistic and technical points of view, of the psychological characteristics of the poetry under consideration here. It is a generalization in which we shall once more find that there is a less absolute line of cleavage between the productions of the modern writers and those of their predecessors than some have imagined.

Except when he is concerned with some purpose extraneous to his art as such—social reform, for example—the poet is delivered, to an extent limited only perhaps by his own personality, from social constraints and demands. He is, therefore, less concerned with the primary aim of communication, namely, to win the co-operation of his readers in some joint activity. It is possible for him, even when he acknowledges the intention to communicate, to allow the act of creation to be in large measure motivated and conditioned by personal satisfactions. In so far as it is so motivated and conditioned, it must frequently become a pure phantasy activity, an excursion in autistic thought. If he chooses to give expression to his autistic thought, we shall have autistic speech in which the artist's lack of corporate feeling and social consciousness will become apparent in the same way as it is manifested in those modes of speech which Piaget has somewhat too readily emphasized as being characteristic of the young child. It will reveal, as we find it doing, the same disregard of the audience without which, however, it would not occur. It will reveal, as we find it doing, the same partial contact with external reality, and it will be characterized in similar fashion by an emotional logic of individual satisfactions, differing widely from that of rational thought. So far as the words are instruments of the fixation of attention, they will appear to be utilized chiefly in concentrating the attention of the writer himself and only secondarily, and perhaps incidentally, to control or fix the attention of others.

Now linguistic development consists, in one of its aspects, in the gradual submission of thought to the reality principle, and in the progressive socialization, which is at the same time the gradual rationalization of thought and speech. Our study appears to show that this trend is reversed in the poetry we have had under consideration, and that the technique of this poetry is broadly, and to a greater degree than ever before, regressive in character. This generalization is borne out also by the syntactical characteristics which we have found to be typical of the verse, by its tendency to juxtaposition, to the omission of expressions which indicate the subtler relations, and towards the paratactic or even the one-word sentence in which the affective, volitional, and cognitive aspects of speech are still only very partially freed from their primitive fusion.

(*Manuscript received* 30 *May* 1936)

PUBLICATIONS RECENTLY RECEIVED

Biological Time. By LECOMTE DU NOÜY. London: Methuen and Co., Ltd. 1936. Pp. x+180. 7*s*. 6*d*.

In this work the author sets out to demonstrate the relation between the sidereal time of the physicist and what he calls physiological or biological time. The book is intended not only for the biologist but also for the lay reader, and hence the first part is devoted to a discussion of biological, chemical and physical methods in general. No criticism can be levied against this part of the work, and these chapters will fulfil their purpose adequately. Part II contains a fairly full discussion of the methods used in, and the results obtained by, studying the cicatrization of wounds in human beings, together with a somewhat shorter outline of tissue culture *in vitro*. In this part of the work an index of cicatrization time is evolved, and it is demonstrated that the influence of age of the patient is such that, in general, it is correct to say that the older the patient the longer will be the time of healing for a wound of given area. In the last part of the book it is shown that the curve connecting age with rate of healing is roughly identical with that connecting age with reciprocal of age. To the psychologist the most disappointing part of this book will be the summary treatment accorded to the interesting question of the manner in which the estimation of long periods of time does vary with age. It is a generally remarked phenomenon that, as age advances, time passes with increasing speed. Here we are told simply that "a year *seems* longer to a child of five than to a man of fifty because it represents the fifth part of his existence, whereas it represents only the fiftieth part of the life of the older man" (pp. 163, 164). This statement clearly specifies the relation between age and estimation of the sidereal year; but for the existence of this hyperbolic relation there appears to be no evidence whatsoever. It seems a pity, therefore, that the argument contained in so much of the later part of the book should depend upon the acceptance of so dubious an assumption. A.F.B.S.

Vom Sinn der Sinne (Ein Beitrag zur Grundlegung der Psychologie). Von ERWIN STRAUS. Berlin: Julius Springer. 1935. Pp. v+314.

In this book Straus has set himself a twofold task: (1) a descriptive analysis of 'Empfinden' (sensing) as contrasted with perceiving and knowing, (2) a refutation of 'Epiphänomenalismus' (parallelism). According to Straus—and contrary to Descartes—'Empfinden' is the "sympathetic intercourse (or communication) of a self with its world", i.e. a 'Totalitätsverhältnis' (relation of totality) of which the individual 'Empfindungen' are modifications (instead of being elements of cognition), and of which the 'Sich-Bewegen' (moving oneself) is the other inseparable side (instead of being accidental). Moreover in the 'Empfinden' there is no such splitting of subject and object as in perceiving and knowing. The world of the 'Empfinden' is *my* fellow world, here and now (not 'an sich'), landscape (not geography), expressive (not objective), enticing and frightening, etc. It would have been in the interest of clarity—though maybe at the cost of originality—if Straus had tried to contrast his views with those of Bergson, Klages and others.

In his polemics against parallelism (including Gestalt-psychology) Straus points out certain characteristics of our phenomenal world which cannot properly be spelled in terms of Pawlow's 'objective psychology', e.g. expecting, seeking, silence, novelty, etc. From this fact he concludes that they cannot be spelled in physiological terms, that the phenomenal order and the physiological order are absolutely incomparable, contrary to the claims of parallelism. What Straus really means by parallelism is best illustrated by his statement that, assuming the proposed literal translation of phenomenal content from physiological process, the occurrence of the latter *within* the organism must needs imply *phenomenal* 'within-ness', i.e. solipsism (pp. 59 f., 159, 214). Thus Straus seems to believe that, according to parallelism, every conceivable predicate of a neural process must have its phenomenal counterpart—which is as strange an allegation as the following one that parallelism has at its disposal no other physiological correlate of 'novelty' than the objective time co-ordinate of the corresponding excitation (pp. 95 ff.). It is a pity that Straus has given so much space to that sort of argument instead of following up the more pertinent question just touched upon on p. 215, namely, what is supposed to be the physiological correlate of this logical structure: "I should have come, if it had not been for my illness"?

Towards a Law of Creative Thought. By ROSAMOND E. M. HARDING. Psyche Monographs: No. 7. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. 178. 6*s*. net.

The general point of view of this book can be stated very briefly. It is maintained that the main developments of knowledge are due to contact between one branch of investigation and another. Such contact is a most powerful aid in the production of new ideas, or combinations of ideas. In the long run any idea will get all the applications of which it is capable, except that checks will arise as a result (1) of inutility and (2) of social stressing. The latter condition means that within a particular area, or at a particular time, certain topical ideas are predominant, and so those that are outside these topics will tend to get neglected. The last section of the book gives a number of interesting instances of the applications of fringe ideas, and shows how such applications have stimulated invention and creation.

The Monograph is not so much one to be read straight through as one to be used by the student who is interested in the psychological study of constructive thinking. It collects together a great many bibliographical references which such a student would find valuable. As a source book containing at the same time a stimulating line of approach the volume should be definitely valuable, and it ought to find a place in all departmental Psychological Libraries.

Statistics for Students of Psychology and Education. By H. SORENSON. London: McGraw-Hill Publishing Co., Ltd. 1936. Pp. viii+373. 21*s*.

This very long presentation of certain elementary statistical methods has little to recommend it specifically to students of Psychology and Education. The usual methods of averaging, assessing variability and correlating, applicable when the population is large, are described, but the student will find little help for dealing with the small samples normally given by laboratory experimentation. The plan of selection of methods to be described was such that eighteen pages are spent on the calculation of the median, barely three pages on the correlation ratio and no mention at all is made of the very valuable χ^2 method. Each chapter is liberally furnished with "Problems, Questions and Exercises", but as no answers are supplied to these they rather fail in their usefulness to an isolated student. A set of useful tables, rather scantily referred to in the text, is given in an appendix.

The exposition is reasonably clear as far as it goes but the redundant style is one calculated to annoy an intelligent student.

Idealogy and Utopia; An Introduction to the Sociology of Knowledge. By KARL MANNHEIM. Translated by Louis Wirth and Edward Shils, with a Preface by Louis Wirth. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. xxxi+318. 15*s*.

This book is an analysis of what Prof. Mannheim believes to be the chief mental patterns underlying social behaviour. Parts II, III and IV are translations of *Idealogie und Utopie*, and Part V is a translation of an article in Vierkandt's *Handwörterbuch* *der Soziologie*, both by Prof. Mannheim, while Part I was written for the present introduction of these works to the English reading public. Part I is a preliminary analysis which shows the relations of the questions to be studied with modern problems of social and political philosophy. Part II studies important changes in the conception of 'Ideology'. Here 'ideologies' are defined as more or less conscious disguises of the real nature of situations, the true recognition of which would be disturbing. There are also the "total" ideologies of a whole age or of a social group or class. Part III deals with the conception of scientific politics, and whether scientific political thought is possible. Part IV deals with the 'Utopian Mentality'. A 'utopia' is here defined as a state of mind which is incongruous with and transcends the immediate situation, but which also tends, by passing over into action, to shatter the prevailing order. In Part V the author attempts a systematic summary and examines the new discipline of the sociology of knowledge.

Undoubtedly this book raises many interesting and important questions, especially about tendencies towards, and resistance of, social change, and about ideas attached to these tendencies in their operation. It also emphasizes important problems about the influence of membership of a given group or set of groups upon the tendencies and ideas of individual members. But it is difficult to believe that such questions can be attacked effectively by social psychology in the absence of very carefully collected and established data. Although this book is relevant to many of the problems of present-day politics and social ideals, a fair criticism would be that it is concerned too much with the analysis of concepts and not enough with the study of data. R. W. P.

The Psychology of Social Norms. By MUZAFER SHERIF. With a foreword by Gardner Murphy. London: Harper and Brothers. 1936. Pp. xii + 209. 6*s*. 6*d*. net.

This book is to be commended to the attention of all psychologists who are interested in some of the wider applications of their subject. The general contention, abundantly and well illustrated, is that social norms, or standards, taken over by men from their social surroundings, are to be regarded as distinct and definite conditions of human reactions. It is further claimed that this fact can be perfectly well illustrated in laboratory experiments, that the growth of such socially determined norms may be watched, and their effects measured. After all that is said about the value and originality of the author's experimental contribution to his problem, the account actually given of it may seem to a good many readers rather an anticlimax. Sherif chose to experiment on the production of subjective rhythms and on the autokinetic illusion. His subjects were tested (*a*) alone and (*b*) in a group situation. Sometimes (*a*) came first and was followed by (*b*) and sometimes (*b*) preceded (*a*). Sherif has no difficulty in showing that the introduction of the group situation affected the experimental results, and how it affected the results, and to some extent the degree to which it affected results. And what he has to say in this connexion is exceedingly interesting, but not so new as he seems to think. Nevertheless the book is a good one, both for students and for teachers. It defines, illustrates and discusses its problems in a clear and direct manner and without any great waste of words. It ought to help to provide a way of working for many investigations, and plenty of problems to which such a way may be applied.

The Nation's Intelligence. By J. L. GRAY. London: Watts and Co. 1936. Pp. 154. 2*s*. 6*d*. net.

The general point of view is that intelligence must not be treated in a static sort of way. Intelligence measures really assess a given individual in terms of his relation to a sufficiently large group of comparable individuals of his own generation. But the general intelligence level may alter from time to time, much as the general standard may alter. And so the problem of the nation's intelligence is always shifting and growing. This point having been made, the author proceeds to sketch the history of investigations of intelligence and to state in simple and untechnical language the main methods and results of contemporary investigations. Throughout he is keenly aware of the importance of a study of social backgrounds in relation to intelligence. The book is, in fact, cautious as well as clear, and both the general reader and the student who is approaching the problems dealt with may be safely advised to read Mr Gray's admirable treatise.

Psychology and the Child. By Ernest G. Braham. London: The Epworth Press. 1936. Pp. 104. 2*s.* 6*d.* net.

Addressed in general to Sunday School workers, teachers, and to parents, this book follows fairly well-marked lines, and cannot be said to contain anything very striking or new. So far as contemporary psychology goes the author has been considerably influenced by Adler. The general reader may find much to interest him, but it is not a book for anybody who has already made some study of psychology.

Towards Peace of Mind; Everyday Problems of Mental Health. By Karl M. Bowman. London: George Allen and Unwin, Ltd. 1936. Pp. 278. 6*s.*

This is a book of very uneven quality. The early chapters on such topics as inheritance and the endocrine glands are interesting and may be supposed to have been of value to the University Extension students to whom the lectures were originally addressed. It is difficult to believe that they profited by the advice given in the more practical parts of the book. Most of it is commonplace; some of it seems to be definitely bad. The attempt to deal with a child's food difficulties by standing over him with a spoon to "shovel it back as fast as he spits it out" seems better calculated to create food difficulties than to cure them. Nor does it seem desirable again to invoke the authority of psychology for the common idea that we should assume that when children cry at nights, they do so in order to dominate the parents. One of the gains of psychoanalytical study of children is that it has been rediscovered that children may cry at nights because they are suffering from anxiety. R. H. T.

A Preface to Nervous Disease. By Stanley Cobb. London: Baillière, Tindall and Cox. 1936. Pp. ix+174. 13*s.* 6*d.*

"The book is written", says the author, "to give to all students of medicine (and all good physicians remain students as long as they live) the facts and correlations needed to understand the simpler workings of the central nervous system." On the whole it may be said to achieve this aim very well indeed. The diagrams and schematizations used—and there are many of them—are remarkably clear, and they are never used without a definite indication that they simplify the actual facts considerably. The general topics dealt with are the autonomic nervous system, segmental aspects of the cerebrospinal nervous system, motor integration and locomotion, cerebral circulation and cerebrospinal fluid, paths of infection in the nervous system, and, in somewhat more detail, histological pathological problems. The chapters on functional localization, on consciousness and the mind-body problem, and on epilepsy and the psychoses, are summary and not so good. But the book as a whole forms a very necessary and good basis for a study in detail of psychological problems of abnormal behaviour.

Psychotherapy. By JACQUES J. COHEN. London: John Bale Sons and Danielsson. 1936. Pp. v+149. 5*s*. net.

This book is written to advance 'psychophonism', which, the author says, "is a word which I have coined to distinguish a psychotherapeutic system of negatival auto-suggestion from all rival systems which, all, start from an entirely wrong foundation, this in their being all alike positival in their psychology". It will be seen that the author's strong point is not lucid English, and in fact the book is extraordinarily difficult to read, mixed up with a tremendous lot of metaphysics, and full of elaborately peculiar phraseology. It seems most difficult to believe that any ordinary psychologist could get much advantage from reading Mr Cohen's work.

Abnormal Personality and Time. By NATHAN ISRAELI. Science Press Printing Company, Lancaster, Pa. 1936. Pp. 123. $2.50.

A brief general introduction indicates that according to several authorities space and time disorders underlie many, or most, physchoses.

Four cases are next studied in detail: the first that of a depressed patient who maintained an unconquerable faith in his power to predict success in games of chance. The second is a paranoid schizophrenic whose delusions were all prophetic and political. The third is that of a maniac patient who "was toyfully preoccupied with concepts about time designated as the 'square of time' and 'circle of eternity', and was also persistently interested in prophecy". The fourth is that of a paranoid schizophrenic who wrote in some detail his own 'future autobiography'.

Thirty-seven paranoid schizophrenics were studied, by an interview and questionnaire technique, at the Worcester State Hospital. Most of them were rather preoccupied with future time factors, though they had not entirely discarded conventional time distinctions. A comparative study of other abnormal patients, carried out with rather elaborate questionnaires, rating-scales and interviews, leads the author to make the interesting suggestion that different types of mental disorder may be characterized by different typical attitudes towards past, present, and especially future events, and to planning in general.

Next, 48 patients at the Bethlem Royal Hospital and 17 mental patients at the Maudsley Hospital were encouraged to write 'future autobiographies'. They had to describe a representative day of their lives a year ahead and 30 years ahead. Again it is indicated that this method may be very useful as a clinical method of differentiation. In general "convalescents have colourless future autobiographies; patients with verbal twists or delusions or negative attitudes write limited autobiographies, or project their distorted feelings or beliefs into their writings".

Superior schoolchildren in Scotland, of ages 12–17, were now given the future autobiography tests: "superior boys have high vocational ambitions and inclinations", while "girls expect their vocational careers to stop at the time of marriage".

Various conclusions are drawn from these comparative studies. Bibliographical references are included.

Wesen und Bedeutung des Schmerzes. Von FERDINAND SAUERBRUCH und HANS WENKE. Berlin: Junker und Dünnhaupt. 1936. Pp. 118.

The little booklet contains the joint attempt of an eminent physician and a philosophical expert to elucidate "the nature and meaning of pain as a uniform phenomenon within the total life of a person", compiling what is known on pain, from neurological and medical facts up to personal attitudes and philosophical interpretations. Its many vaguenesses are somewhat balanced by the novelty of treating the subject from so many different aspects.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

1936

GENERAL MEETINGS

March 7, 1936.	A Symposium on "Appetites." Papers by: Prof. David Katz. C. A. Mace, M.A. Dr J. T. MacCurdy.
March 20, 1936.	General Meeting, followed by Lecture. (See under Lectures.)
April 17–20, 1936.	Extended General Meeting at Leeds.
April 17.	1st Session: "A Spatial Sub-Test of Intelligence." By Ll. Wynn Jones, M.A., B.Sc., Ph.D. "The Later Development of Clever Children who failed to secure Admission to Secondary Schools." By E. H. Axton, M.A. 2nd Session: "Methods of Investigation in Psychology and Social Anthropology." By Miss E. J. Lindgren, M.A., Ph.D.
April 18.	3rd Session: Symposium on "Colour Constancy." Papers by: Prof. David Katz. R. H. Thouless, M.A., Ph.D. T. M. Martin, M.Sc. (Afternoon): Visit to New University Library and Demonstration of the Oregon Test of Musical Discrimination. 4th Session: "Testimony concerning Heard Conversation." By Prof. T. H. Pear.
April 19.	5th Session: "Psychology of Being." By Dr Oswald Schwarz. 6th Session: "The Diagnostic Value of Reaction to Failure." By Miss D. Gandine-Stanton, M.Sc. 7th Session: "Some Psychological Problems of the Business Executive." By A. H. Seymour, B.Sc.
May 15, 1936.	General Meeting, followed by Lecture. (See under Lectures.)
June 19, 1936.	General Meeting, followed by Lecture. (See under Lectures.)

June 27, 1936. Papers:

"The Time Factor in Children's Memory Drawings." By Miss V. N. Hall, M.A.

"A Series of Perceptual Tests." By John C. Raven, M.Sc.

Demonstrations:

"The Effects of Conation upon Subliminal Auditory Stimuli." By M. F. Lowe, Ph.D.

"A Tachistoscope for the Study of the ϕ Phenomenon." By A. M. Uttley, B.Sc.

November 28, 1936. Joint Meeting with the Cambridge Psychological Society, at Cambridge.

Papers:

"Some Experiments bearing on 'Internal Inhibition'." By R. C. Oldfield, M.A.

"Social Grouping in Relation to Class Stratification." By R. H. Thouless, M.A., Ph.D.

Demonstrations:

Apparatus designed by Dr Schuster for use in Industrial Psychology. By E. Farmer, M.A., E. G. Chambers, M.A. and J. G. Friedeberg.

Experiment on the Effect on Learning of Knowledge of Results. By G. C. Grindley, M.A.

Experiments on Factors influencing Accuracy of Testimony. By Miss M. Harvey and Mrs H. L. Elvin, B.A.

Experiments on Remembering a Psychological Discussion. By Miss E. J. Lindgren, M.A., Ph.D. and J. M. Blackburn, B.Sc., Ph.D.

Experiments on Skin Temperature Sensitivity. By Dr J. T. MacCurdy.

Experiment on Habituation of the Blink Response. By R. C. Oldfield, M.A.

Experimental Deafness. By A. F. Rawdon Smith, Ph.D.

Experiments on Phenomenal Regression. By R. H. Thouless, M.A., Ph.D.

Experiment on the Visual Estimation of Space. By Miss M. D. Vernon, M.A.

Experiments on Perception and Recognition. By O. L. Zangwill, B.A.

Some new pieces of Classroom Apparatus. By K. J. W. Craik, M.A., A. Crowther, B.A., A. T. Welford and W. S. Robertson.

December 12, 1936. Annual General Meeting.

"The Sensory Element in Manual Skill." By G. H. Miles, D.Sc. and L. S. Hearnshaw, M.A.

"The Present Position of Vocational Psychology." By T. A. Rodger, M.A.

LECTURES

March 20, 1936.	"Temperaments, Physical and Statistical." By Prof. M. Greenwood, F.R.S.
May 15, 1936.	"Doctrines of Formal Discipline: Ancient and Modern." By Prof. H. R. Hamley.
June 19, 1936.	"Mental Health and Mental Sickness." By Dr J. R. Rees.

SECTIONAL MEETINGS

Medical

January 22, 1936.	Address from the Chair: "Post-War Social Activities and Advances in Psychotherapy." By Dr Sylvia Payne.
February 26, 1936.	"The Role of the Psychiatric Social Worker." By Miss Margaret Ashdown, M.A.
March 25, 1936.	"The Importance of Dream Analysis for Psychological Development." By Dr H. Godwin Baynes.
April 22, 1936.	"Thirty Years of Psychotherapy." By Dr Maurice B. Wright.
May 27, 1936.	Joint Meeting with the Royal Anthropological Institute on "Patterns of Culture." Opening paper by Prof. C. G. Seligman. Papers by: Miss Audrey Richards, M.A., Ph.D. (for the R.A.I.). Prof. J. C. Flugel (for the Medical Section).
June 24, 1936.	"Somnifaine Narcosis." By Dr Rowland Hill.
October 28, 1936.	Joint Meeting with the Aesthetics Section on "The Nature of Ugliness." Papers by: J. S. Barwell, M.A. (Aesthetics Section). Dr John Rickman (Medical Section).
November 25, 1936.	"Some Clinical Observations on Schizophrenic Drawings." By Dr E. Guttmann (in collaboration with Dr W. S. Maclay).
December 16, 1936.	Joint Meeting with the Institute for the Scientific Treatment of Delinquency on "The Unwilling Patient." Papers by: Dr Denis Carroll, Dr O. H. Woodcock } for the Institute. Dr W. H. de B. Hubert, Dr J. R. Rees } for the Medical Section.

Education

February 10, 1936.	"An Attempt to Investigate the Growth of Sentiments." By Miss Margaret Phillips, M.A.
March 14, 1936.	Four Short Papers on Various Aspects of Backwardness in Arithmetic. 1. "Emotional Aspects of Backwardness in Arithmetic." By Miss M. MacTaggart, M.A., B.Ed., Ph.D.

	2. "Intellectual Aspects of Backwardness in Arithmetic." By C. A. Claremont, B.Sc., A.C.G.I.
	3. "Backwardness in Arithmetic in the Junior School." By S. H. Cracknell, M.Sc.
	4. "Backwardness in Number in the Infants' School." By Miss F. E. Roe.
June 8, 1936.	"Mental Hygiene of the Elementary School Child." By Dr Mildred Creak.
November 9, 1936.	"Problems of the Brilliant Child." By Miss E. Mildred Nevill, Dip.Psy.
December 15, 1936.	"Backwardness in English Composition." By F. J. Schonell, Ph.D.

Industrial

January 31, 1936.	"What is Accident Proneness?" By E. G. Chambers, M.A.
March 25, 1936.	"The Difficulty of Measuring Learning by Learning Curves." By J. M. Blackburn, B.Sc., Ph.D.
April 29, 1936.	"The Work of a Secretary to a Juvenile Advisory Committee of the Ministry of Labour." By Miss Jean A. Wales.
May 26, 1936.	"Psychological Factors in Industrial Sickness." By Dr T. M. Ling.
June 15, 1936.	"Handwriting as a Means of Psychodiagnosis." By Jan Meloun.
October 26, 1936.	"External Evaluations of Industrial Psychology." By Prof. G. R. T. Ross.
November 18, 1936.	"Some Physiological Aspects of the Noise Problem." By Dr G. P. Crowden.

Aesthetics

January 24, 1936.	"Popular Expectations from Entertainment." By D. W. Harding, M.A.
February 28, 1936.	"Pictures and the Psycho-galvanic Reflex." By R. J. Bartlett, M.Sc., A.R.C.S.
March 19, 1936.	A group discussion on "The Nature of Ugliness." Opened by Prof. L. S. Stebbing and C. A. Mace, M.A.
May 15, 1936.	"Art in Primitive Society." By Raymond Firth, M.A., Ph.D.
July 8, 1936.	"Psychological Aspects of Fashion and Advertising." By Prof. T. H. Pear.
October 28, 1936.	Joint Meeting with the Medical Section on "The Nature of Ugliness." Papers by: J. S. Barwell, M.A. (Aesthetics Section). Dr John Rickman (Medical Section).

BRANCH MEETINGS

Session 1935–6

Scottish Branch

November 2, 1935.	(Edinburgh.) "Practical Problems in the Borderland between Logic and Psychology." By R. H. Thouless, M.A., Ph.D. "The Assessment of Teaching Ability." By J. L. Hardie, M.A., Ed.B.
December 7, 1935.	(Glasgow.) "The Place of the Conditioned Reflex in Psychological Theory." By Dr D. G. Marquis (Yale). "Biological and Gestalt Views of Group Psychology." By R. W. Pickford, M.A., Ph.D.
January 25, 1936.	(Edinburgh.) "A Time-table of the Emergence of Instinct Activities." By Dr E. H. Connell. "Dreams and Phantasies drawn by Children showing different Behaviour Abnormalities." By J. D. Sutherland, B.Sc., B.Ed., Ph.D.
March 7, 1936.	(Glasgow.) "Is Perseveration a Fluctuating Function?" By W. Logan (Glasgow). "The Connection between Oscillations in Short-period efficiency and Steadiness of Character." By R. D. Walton (Glasgow). "Notes on the Dating of Words." By Dr J. H. Kenneth.
May 9, 1936.	(St Andrews.) "The Vagaries of an Emancipated Psychology." By Prof. J. Drever. "Aspects of Child Guidance." By N. T. Walker, M.A., B.Ed., Ph.D. "Child Guidance in Italy." By A. R. Knight, M.A.

Midland Branch

November 27, 1935.	Annual General Meeting. "Temperament Tests in Clinical Practice." By R. B. Cattell, Ph.D.
January 29, 1936.	"Mental Association and Perseveration in the light of Lindworsky's Theory of Mental Resonance." By Rev. J. Leycester King, S.J., D.Phil.
February 19, 1936.	"Psychology and the Literature of To-day." By W. H. Auden.
March 18, 1936.	"Birmingham Experiments in Vocational Guidance and Selection." By Mrs E. P. Hunt.

Northern Branch

April 17–20, 1936.	Extended General Meeting of the Society at Leeds. (Arranged with the co-operation of the Northern Branch.) (See under General Meetings.)

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS

4. The Society shall consist of Honorary Members, Ordinary Members, Branch Associates and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 *a*. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish

information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society sum

moned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Deputy President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, one representative of the Committee for Research in Education to be nominated by that Committee, and of six Ordinary Members, and not more than two members co-opted by the Council.

22. The President and Deputy President of the Society shall hold office for three years. The other officers of the Society shall retire annually but shall be eligible for re-election, the Deputy President shall be eligible only once for re-election, and the President shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. Co-opted members of the Council shall cease to hold office at the Annual General Meeting of the year of their election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 *a*. Nominations of representatives of the Sections, the Branches and the Committee for Research in Education shall be sent to the General Secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council and Committees or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 21, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend, gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 2*s*.

36. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

37. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, Deputy President, or failing them a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President, Deputy President, or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

46 *d*. Branches shall have the right of electing Branch Associates under conditions similar to those governing the election of Sectional Associates.

RULES

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

BRITISH PSYCHOLOGICAL SOCIETY

LIBRARY RULES

1. The Library of the Society shall ordinarily be open for the use of members from 10 a.m. to 5 p.m. (Saturdays, 10 a.m. to 1 p.m.).

2. Persons other than members of the Society may use the Library only on obtaining permission from the Hon. Librarian. Such permission shall as a rule be granted only to scientists or research students who are known personally or by reputation to the Hon. Librarian, or who are introduced to him by a member of the Society. Such persons must in all cases have declared their willingness to conform to the rules regulating the use of the library by members.

3. Members must on each visit to the library sign the book provided for that purpose.

4. No book may be marked, annotated, mutilated, or defaced in any way.

5. A member shall be responsible to the Society for any loss or damage occurring to a book while in his use or possession.

6. No books may be removed or borrowed from the library.

LIST OF PERIODICALS AND SERIAL PUBLICATIONS CONTAINED IN THE LIBRARY (1935)

* Denotes publications from year given up to date.

*Acta Psychologica. 1935–
*Action et Pensée (Bulletin de la Société Internationale de Psychagogie et de Psychothérapie). 1925–
American Journal of Insanity. 1903–1915.
*American Journal of Psychiatry. 1921–
*American Journal of Psychology. 1888–
*Annales médico-psychologiques. 1921–
*l'Année psychologique. 1909–
*Archiv für die gesamte Psychologie. 1903–
Archiv für Gewerbepathologie und Gewerbehygiene. 1930–1932.
Archiv für Kriminologie. 1922–1930.
*Archives de psychologie. 1902–
*Archives of Neurology and Psychiatry. 1919–
*Archives of Psychology. 1907–
*Archivo generale di neurologia e Psychiatria. 1921–
*Australasian Journal of Psychology and Philosophy. 1923–
Berichte über den 1–9 Kongress für experimentelle Psychologie. 1904–1924.
Brain. 1914–1921.

*British Journal of Educational Psychology. 1931–
*British Journal of Medical Psychology. 1920–
*British Journal of Psychology (General Section). 1904–
*British Journal of Psychology Monograph Supplements. No. 1–
Canadian Journal of Mental Hygiene. 1919–1922.
*Character and Personality. 1932–
Columbia University Contributions to Education. Selected numbers.
*Comparative Psychology Monographs. 1922–
Die Experimentelle Pädagogik. 1905–1910.
*Educational Research Bulletin. 1929–
*l'Encéphale. 1927–
*Eugenics Review. 1923–
Fortschritte der Psychologie. 1913–1922.
Forum of Education. 1923–1930. (Now incorporated with the British Journal of Educational Psychology.)
*Genetic Psychology Monographs. 1927–
Grenzfragen des Nerven- und Seelenlebens. 1900–1914, 1917–1927.
*(The) Human factor. (Journal of the N.I.I.P.) 1932–
*Imago. 1913–
*Industrial Health Research Board Reports. No. 1–
Industrial Psychology. 1926–1928.
*Industrielle Psychotechnik. 1924–
*International Journal of Psycho-Analysis. 1920–
*International Zeitschrift für Individualpsychologie. 1924–
*Internationale Zeitschrift für Psychoanalyse. 1913–
Journal de Neurologie et Psychiatrie. 1927–1929.
*Journal de Psychologie. 1904–
*Journal für Psychologie und Neurologie. 1902–
*Journal of Abnormal and Social Psychology. 1907–
Journal of Animal Behavior. 1911–1917.
*Journal of Applied Psychology. 1917–
Journal of Comparative Neurology and Psychology. 1907–1910.
*Journal of Comparative Psychology. 1921–
*Journal of Educational Psychology. 1910–
*Journal of Educational Research. 1920–
Journal of Experimental Pedagogy. 1911–1921.
*Journal of Experimental Psychology. 1916–
*Journal of General Psychology. 1928–
*Journal of Genetic Psychology (formerly Pedagogical Seminary). 1891–
*Journal of Juvenile Research. 1929–
*Journal of Mental Science. 1921–
*Journal of Nervous and Mental Disease. 1926–
*Journal of Neurology and Psychopathology. 1920–
*Journal of Philosophical Studies (now Philosophy). 1926–
*Journal of Philosophy. 1911–
Journal of Philosophy, Psychology and Scientific Methods. 1908–1914.
*Journal of Social Psychology. 1930–

*Journal of the National Institute of Industrial Psychology (now The Human Factor). 1924–
*Journal of the Royal Anthropological Institute. 1909–
*Kwartalnik Psychologiczny. 1930–
Man. 1906–1928.
*Mind. 1915–
*Pedagogical Seminary and Journal of Genetic Psychology (now Journal of Genetic Psychology). 1891–
*Personnel Journal. 1927–
*Philosophical Review. 1915–
Philosophische Studien (Wundt). 1883–1902.
*Philosophy (formerly Journal of Philosophical Studies). 1926–
Praktische Psychologie. 1919–1922.
Proceedings of the Aristotelian Society. 1916–1923.
*Proceedings of the National Academy of Science. 1929–
*Proceedings of the Society for Psychical Research. 1910–
*Psyche. 1921–
*Psychoanalytic Quarterly. 1932–
*Psychoanalytic Review. 1913–
die Psychoanalytische Bewegung. 1929–1933.
*Psychological Abstracts. 1927–
*Psychological Bulletin. 1904–
*Psychological Clinic. 1908–
Psychological Register. 1929, 1932.
*Psychological Review. 1894–
*Psychological Review Index. 1894–
*Psychological Review Monographs. No. 1–
Psychologische Arbeiten (ed. Kräpelin). 1895–1928.
*Psychologische Forschung. 1922–
Psychologische Studien (Wundt). 1906–1917.
*Revue Française de Psychanalyse. 1927–
*Rivista di Neurologia. 1928–
*Rivista di Psicologia. 1905–
Science Progress. 1917–1933.
*Scientia. 1926–
Training School Bulletin. 1929–1931.
*le Travail Humain. 1933–
*University of California Publications. 1916–
Yale Psychological Laboratory Studies. 1892–1902.
*Zeitschrift für angewandte Psychologie. 1908–
Zeitschrift fur angewandte Psychologie. Beihefte.
Zeitschrift für Neurologie und Psychiatrie. 1910–1914.
*Zeitschrift für pädagogische Psychologie. 1899–
*Zeitschrift für psychoanalytische Pädagogik. 1926–
*Zeitschrift für Psychologie. 1890–
Zeitschrift für Psychotherapie und medizinische Psychologie. 1909–1915.
*Zeitschrift für Sinnesphysiologie. 1908–

In addition to the above, the Library contains a few numbers only of the following publications:

Arbeiten aus dem Psychologischen Institut der Universität München.
Archivos Brasileiros de Hygiene Mental.
Bulletin de l'Institut Général Psychologique.
l'Hygiène Mentale. (1927.)
Indian Journal of Psychology.
Journal of the Department of Letters, University of Calcutta.
Mental Measurement Monographs.
Nervous and Mental Disease Monograph Series (selected numbers).
Pädagogisch-psychologische Arbeiten (aus dem Institute des Leipziger Lehrervereins).
Scandinavian Scientific Review.
Smithsonian Institution Publications.
Tohoku Psychologica Folia.
Travaux Psychologiques de l'Université Jean Casimir de Lwów, Poland.